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RIPARIA BRIDGE, SNAKE RIVER.

WE publish illustrations, for which we are indebted to *Engineering*, of an interesting structure recently completed by Mr. George S. Morison, of Chicago, over the Snake River, at Riparia, for the Oregon Railway and Navigation Company. The bridge crosses the Snake River about seventy-five miles above the confluence of the Snake and Columbia Rivers in Washington. The piers of the bridge are built of basaltic stone, the two abutments being founded on piles, and the three river piers on pneumatic caissons. The only original feature of the bridge is the turntable of the draw span. The entire weight of the draw is carried on two cross girders which are connected together. The girders are carried on the plunger of a central hydraulic press. When the bridge is closed the weight of the trusses is carried directly to the masonry by side bearings, on which cross girders rest. When the bridge is to be opened the cross girders are raised by the hydraulic press until the side bearings are entirely clear, and then the bridge turns easily on the fluid center. In turning, the draw is guided and kept level by eight wheels with heavy spring bearings, the springs being strong enough to prevent any rocking of the draw when turning, and not so stiff but what they are compressed when the bridge is lowered on the side bearings. Fig. 1 is a general view of the bridge with the draw span closed and Fig. 2 a similar view with the span open. Fig. 3 shows the pier of the draw span with the substructure on which the span rents; Figs. 6 to 37 are details of this substructure. Fig. 3 gives a very clear idea of the arrangement referred to above; in this view *a* and *b* are the two central girders connected together, and the ends of which are supported on wheels; *d* and *e* are the outer brackets, also supporting the girders and carried on wheels; and *e e* are two other girders connecting the whole structure. Figs. 4 to 7 are details of girders *a* and *b*, other views of which are given in Figs. 19, 20, and 21; Figs. 9 to 15 are details of bracket girders, *d* and *e*; and Figs. 16 and 17 are girders, *c*. Fig. 30 is a section of the central ram, shown to a smaller scale in Figs. 19 to 21, and Figs. 23, 25, and 26 are views of the cart saddle that rests on the hemispherical head of the ram. Figs. 35 and 36 are views of the carriage on which the ends of the girders, *a* and *b*, and the bracket girders rest; and Figs. 31 to 34 are details of the locking gear. The Figs. 19 to 21 show the general arrangement of the turning gear, and a part of the internal toothed ring, and the driving pinion is given in Fig. 37. Appended is a copy of the specification under which the Riparia Bridge was constructed:

General Description.—The superstructure will consist of two main through spans and one draw.

Each through span will be 325 ft. long between centers of end pins, divided into thirteen panels of 25 ft. each. The trusses will be 40 ft. deep, and placed 18 ft. apart between centers. Each span will weigh approximately 700,000 lb.

The draw will be 352 ft. long between centers of end pins and be carried on a center bearing table.

Plans.—Full detail plans, showing all dimensions, will be furnished by the engineer. The work shall be built in all respects according to these plans. The contractor, however, will be expected to verify the correctness of the plans, and will be required to make any changes in the work which are necessitated by errors in these plans, without extra charge, where such errors could be discovered by an inspection of the plans.

Materials.—All parts, except nuts, swivels, clevises, wall pedestal plates, and ornamental work, will be of steel. The nuts, swivels, and clevises may be of wrought iron; the pedestal plates and ornamental work will be of cast iron.

All materials shall be subject to inspection at all times during their manufacture, and the engineer and his inspectors shall be allowed free access to any of the works in which any portion of the material is made. Timely notice shall be given to the engineer, so that inspectors may be on hand.

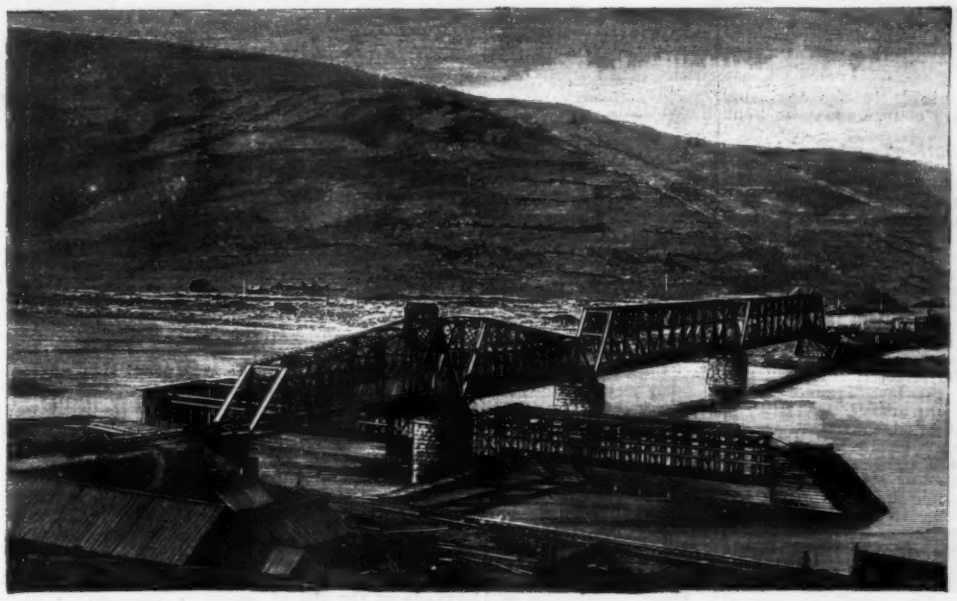


FIG. 1.

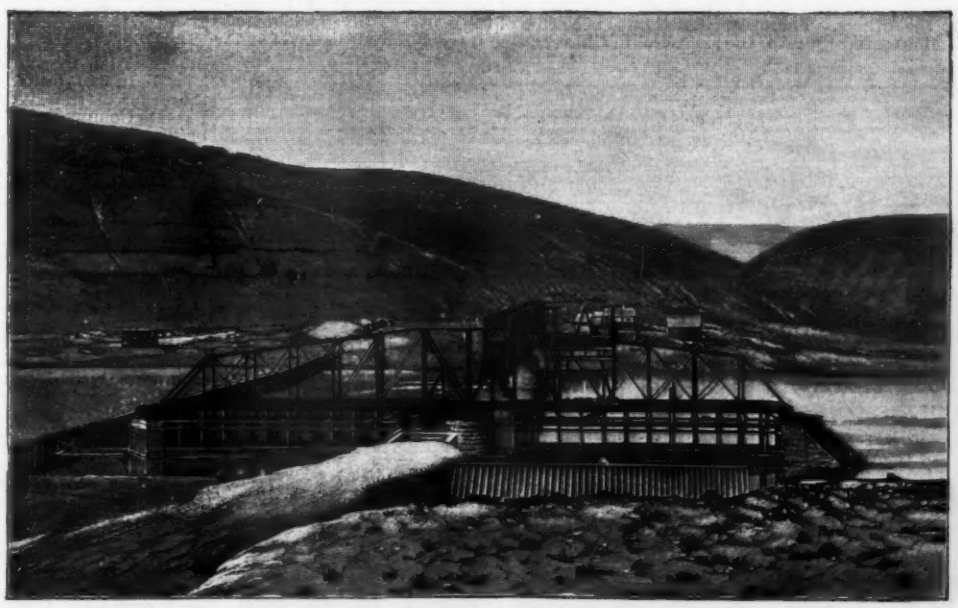


FIG. 2.

BRIDGE OVER THE SNAKE RIVER AT RIPARIA, WASHINGTON.

Steel may be made by the open hearth or by the Bessemer process, but no steel shall be made at works which have not been in successful operation for at least one year. Steel made by the Clapp-Griffiths process will not be accepted. All melts shall be made from uniform stock low in phosphorus, and the manufacturer shall furnish satisfactory evidence to the engineer that this class of material is being employed, it being understood that the finished product of open hearth steel is to be one in which the phosphorus does not average more than $\frac{8}{100}$ of one per cent., and never exceeds $\frac{1}{10}$ of one per cent., and in the finished product of Bessemer steel the average of phosphorus shall not be more than $\frac{4}{100}$ of one per cent. and the maximum not more than $\frac{1}{10}$ of one per cent.

A sample bar having 1 square inch cross section shall be cut from the finished product of every melt. The laboratory tests shall be made on this sample bar in its natural state, without annealing. The laboratory tests of steel made on the sample bar shall show an elastic limit of not less than 38,000 lb. per square inch, an ultimate strength of not less than 63,000 lb., nor more than 70,000 lb.; an elongation of at least 22 per cent. in a length of 8 in.; and a reduction of at least 45 per cent. at the point of fracture; this elongation and reduction being the minimum and not the average requirements. In a bending test the sample bar shall bend 180 deg. and close back against itself without showing crack or flaw on the outside of the curve. Steel having an ultimate strength of 60,000 lb. per square inch will be accepted for rivets.

Should the contractor desire to use British steel, the quenching and bending tests specified in the Hawkesbury Bridge specifications will be required, and the elastic limit requirement may be waived. Every piece of steel shall be stamped with a number identifying the melt, and a statement of the results of the laboratory tests of each melt shall be furnished by the contractor, certified by some person acceptable to the engineer, and accompanied by the tested specimens. Should the manufacturers make tests of bars rolled from sample ingots, certified copies of the report of said tests shall also be furnished to the engineer.

All sheared edges or punched holes in steel work shall be subsequently planed or drilled out, so that none of the rough surface is ever left upon the work. Steel for pins shall be sound and entirely free from piping, and pins more than 4 in. in diameter shall be drilled through the axis. Nuts, swivels, and clevises shall be strong enough to break the members to which they are attached.

Cast iron shall be of the best quality of tough gray iron.

Riveted Work.—Plates, angles, and channels shall be carefully straightened before they are laid out; the rivet holes shall be carefully spaced in truly straight lines; the rivet heads shall be of hemispherical pattern, and the work shall be finished in a neat and workmanlike manner. Surfaces in contact shall be painted berfore they are put together. The dimensions given for rivets on the plans are the diameters of the rivets before driving.

Power riveters shall be direct-acting machines, capable of exerting a yielding pressure, and holding on to the rivet when the upsetting is completed.

The several parts of each member shall be assembled and the holes shall be drilled, the sharp edge of the drilled hole shall be trimmed so as to make a slight fillet under the rivet head, and the pieces shall be riveted together without taking apart.

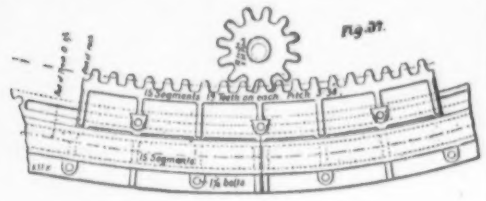
Should the contractor desire, the parts may be punched with a punch at least $\frac{1}{16}$ in. smaller than the diameter of the rivet as given on the plans, working in a die only $\frac{1}{16}$ in. larger than the rivet; the several parts of the member shall then be assembled and the holes reamed so that at least $\frac{1}{16}$ in. of metal is taken out all around, the sharp edge of the reamed hole shall be trimmed and the pieces riveted together as above. All rivets shall be of steel; the rivet holes shall be of such size that they will fill the hole before driving, and whenever possible, the rivets shall be driven by power. All bearing surfaces shall be truly faced. The chord pieces shall be fitted together in the shop in lengths of at least five panels, and marked. When so fitted there shall be no perceptible wind in the length laid out. The pin holes shall be bored truly, so as to be at exact distances, parallel with one another, and at right angles to the axis of the member.

The pin holes in the vertical posts shall be truly parallel with one another, and at right angles to the axis of the posts. The posts shall be straight and free from wind.

The holes for the rivets connecting the floor beams with the posts and bolsters and the stringers with the floor beams, and, in general, the holes for all rivets which must be driven after erection, shall be accurately drilled to an iron templet. The holes for the rivets connecting the floor beams with the posts shall be 1 in. in diameter, and the rivets of corresponding diameter.

Forged Work.—The heads of eye bars shall be formed by upsetting and forging into shape by such process as may be accepted by the engineer. No welds will be allowed. After the working is completed, the bars shall be annealed by heating them to a uniform dark red heat throughout their entire length, and allowing them to cool slowly. The form of the heads of steel eye bars may be modified to suit the process in use at the contractor's works, but the form of the head adopted must be such as to meet the requirements of the tests of full sized bars.

The heads and the enlarged ends for screws in laterals, suspenders, and counters shall be formed by



upsetting by a process acceptable to the engineer. The eyes on the 1 inch square counters may be bent around and welded.

Tests of Full Sized Steel Bars.—Ten full sized eye bars of sections and lengths used in the actual work shall be selected from bars made for the bridge, by the inspector for testing. Each of these full sized bars shall be strained till an elongation of 10 per cent. is obtained, and, if possible, broken. If broken, the fracture shall occur in the body of the bar, and shall show a uniform and ductile quality of material.

The contractor will be required to send the full sized bars to Athens, Pa., to be broken in the testing machine at that place, unless some other machine has been specially approved for this purpose by the chief engineer.

The full sized bars shall be selected from time to time as the work proceeds, the last bar not to be selected till all the eye bars are manufactured. The tests shall be made from time to time as the bars are selected. When three bars have been tested, the bars manufactured up to the time of the selection of these three test bars shall be accepted or rejected on the results of such tests, and the same shall be done again when three more bars are tested. In these tests the failure of one bar to develop a stretch of 8 per cent., or of the lot to develop an average of 10 per cent. before breaking, shall be sufficient reason for rejecting the lot from which these bars are taken. A failure to break in the body of the bar shall not be a sufficient ground for condemnation if it does not occur in more than one-third of the bars tested; but the above requirements as to elongation shall apply to the bars so breaking in the head, as well as to the others. The chief engineer shall, however, examine carefully into the cause of breakage of any bar which does not meet the requirements, and, if the defect is explained, may order additional tests, and make the acceptance dependent on further results.

Machine Work.—The bearing surfaces in the top chord shall be truly faced. The ends of the stringers and of the floor beams shall be squared in a facer. All surfaces so designated on the plans, shall be planed. All sheared and punched edges shall be planed or bored out.

All pins shall be accurately turned to a gauge and shall be full size throughout. Pin holes shall be bored to fit the pins, with a play not exceeding one-fiftieth inch. These clauses apply to all lateral connections as well as to those of the main trusses. Pins shall be supplied with pilot nuts, for use during erection, four for each size of pin.

All screws shall have a truncated V thread, United States standard sizes.

Draw.—The draw will include the suppporting girders, rods, and frames at the center and the lifting and latching apparatus at the ends. The center on which the bridge turns, the bearing wheels and the circular track, are not included in this contract.

These specifications shall apply to all the material in this draw in the same manner as the fixed spans. The latching arrangement at the ends shall be finished exactly according to the plans furnished.

Miscellaneous.—All workmanship and material, whether particularly specified or not, must be of the best kind now in use in first-class bridge work. Flaws, ragged edges, surface imperfections or irregular shapes will be sufficient ground for rejection. Rough and irregularly finished work will not be accepted.

Machine-finished surfaces shall be coated with white lead and tallow before shipment. All other parts shall be given a coat of Cleveland ironclad paint, purple brand.

Terms.—The prices for the work shall be by the pound at separate rates for steel and cast iron. The price for wrought iron shall be the same price as for steel. The prices shall be for material on cars at Chicago.

Monthly estimates will be made at the end of each month for the work done during that month. In these monthly estimates, the material delivered at the contractor's shop, but not manufactured, shall be estimated at 50 per cent. of the contract price for finished material in Chicago, and manufactured material at 75 per cent. of the contract price for finished material in Chicago. Payments will be made on or about the fifteenth day of the following month, according to these estimates, deducting from the amount of the same 10 per cent. as security, to be held until the completion of the entire contract.

No material will be paid for which does not form a part of the permanent structure.

All expenses of testing shall be borne by the contractor.

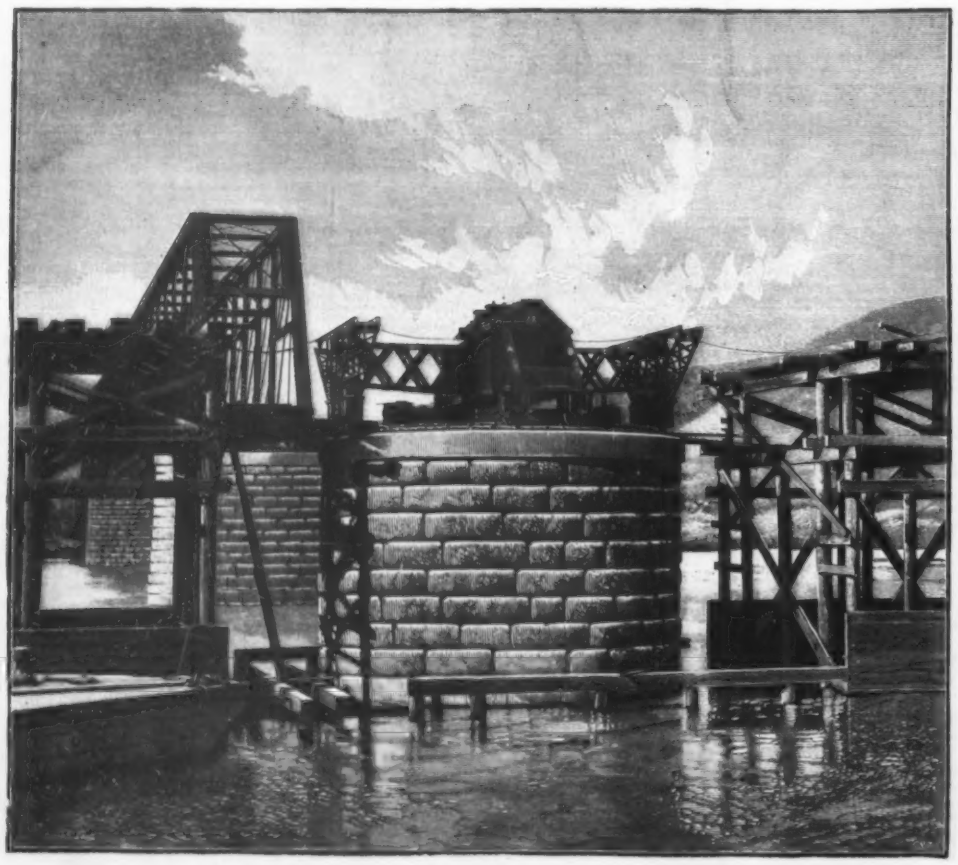
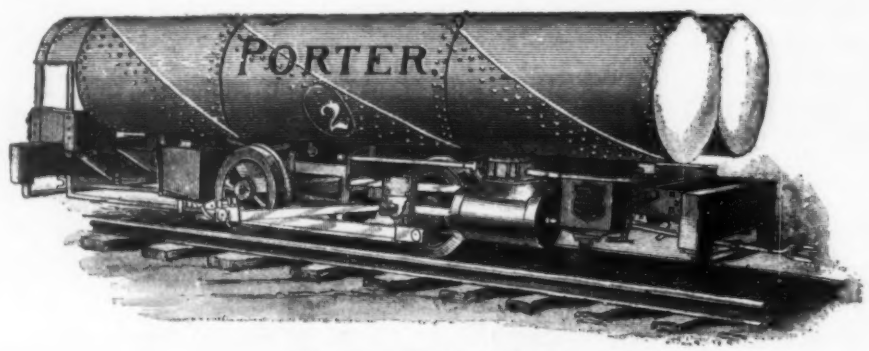


FIG. 3.

BRIDGE OVER THE SNAKE RIVER AT RIPARIA, WASHINGTON.

COMPRESSED AIR MINE LOCOMOTIVES.

AN interesting engine operated by compressed air, for use in mines, has been constructed by H. K. Porter & Co., of Pittsburg, Pa. This engine has cylinders eight inches in diameter, with fourteen inch stroke. It has four driving wheels twenty-four inches in diameter, and a wheel base of five feet. The engine has two tanks, thirty-three inches by sixteen feet. The weight



COMPRESSED AIR ENGINE.

of the engine is 16,000 pounds. It can haul thirty coal cars or a train of about sixty tons up a 3 per cent. grade. Three miles can be made without recharging with air. The air is charged at from 250 to 450 pounds pressure. There is no trouble of any kind from freezing. The engine can be charged, allowed to stand a month and yet start off when wanted. This is no experiment. This type of low motion has been in constant economical operation for a number of years.

This kind of an engine is much easier to handle than the usual type of steam locomotive. It can be built for any gauge track, and for any sized tunnel within reasonable limits. Cylinders are made in several sizes, from five by ten inches up. An engine of this type can be used under any conditions where it is undesirable to have fire present. Another use for which an engine of this type is peculiarly adapted is that of street railways. It has none of the objectionable features of electricity and is cheaper, a consideration well worth weighing.—*Jour. Ry. Appliances.*

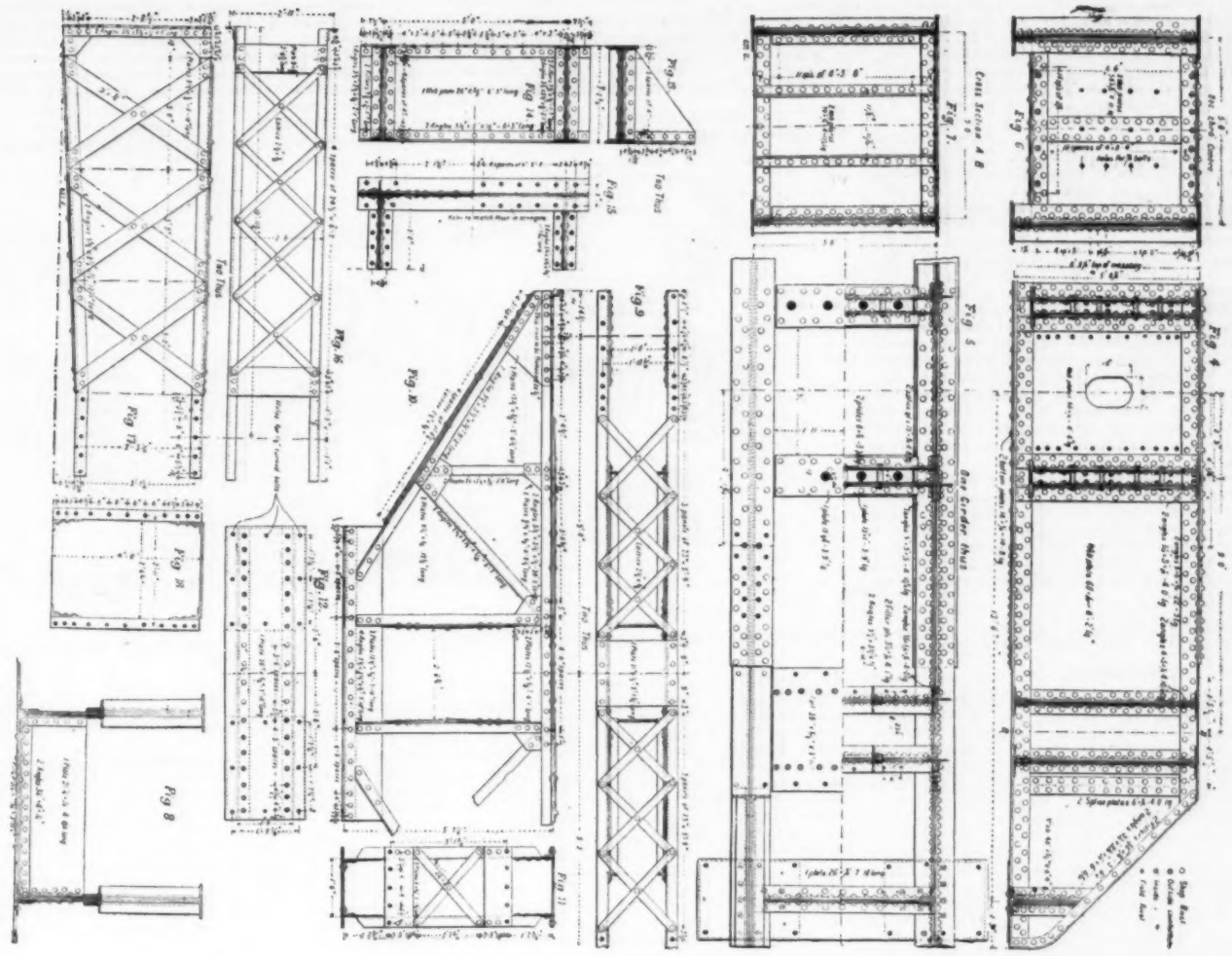
MACHINE RIVETING SHIPBUILDING.

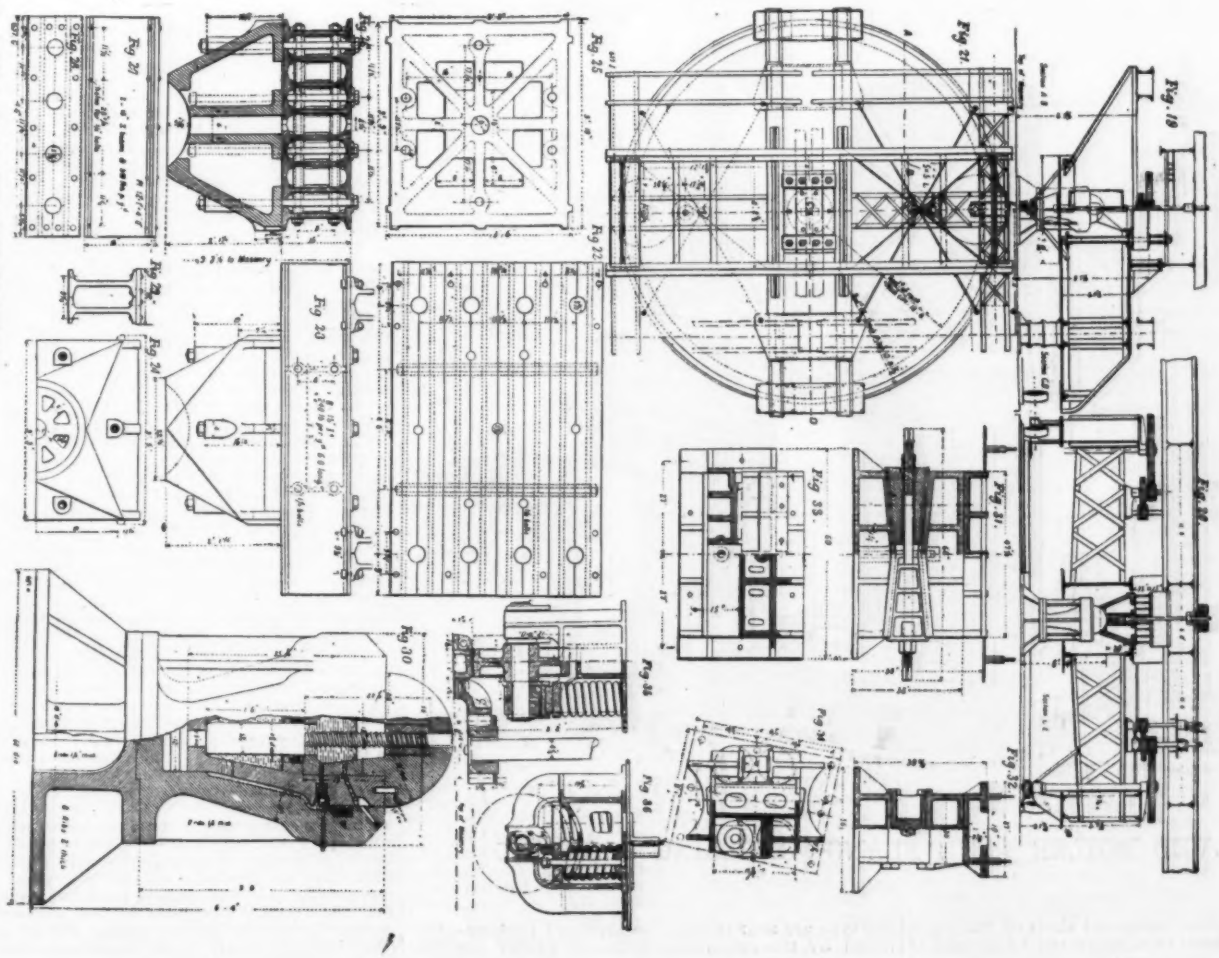
A NUMBER of gentlemen lately visited the shipbuilding yard of Messrs. Swan & Hunter, at Wallsend, Newcastle, to inspect a vessel built upon the principle of Mr. A. C. Holzapfel. The vessel was a steel barge, named the Alpha, and intended for use on the Thames. Her dimensions and description are thus detailed by the *Engineer*. A steel barge, 72 ft. long, 19 ft. 6 in. beam, and 7 ft. depth of hold, built to the order of the Hydraulic Appliances Shipbuilding Patents Company, of London. This vessel has been built to test Mr. A. C. Holzapfel's patent method of shipbuilding by hydraulic machinery The method consists of flanging the plates on all four sides, then placing them in position, and then punching and riveting the flanges by traveling hydraulic machinery. The plates used for this purpose are 5 ft. long and 2 ft. wide, and ¼ in. thick, and they were flanged at the hydraulic press of Messrs. Robert Stephenson & Co., Forth Banks, Newcastle, under the supervision of the inventor. They were then sent to Messrs. Swan & Hunter's, where they were punched and riveted together as indicated. The flanges are 4½ in. deep, and give a continuous keelson running fore and aft to each width of plate, or at intervals of 2 ft., from which the vessel undoubtedly derives great longitudinal strength. In order to provide suitable means of drainage from the various cells formed of the flanged plates in the bottom Goodall's patent tube rivets were used and riveted within ½ in. of the root of the flanges. The cells were then cemented level with the bottom of the tube rivets, giving continuous arrangements for draining throughout the bottom. The vessel has stringer plates 15 in. wide throughout the sides running fore and aft, and has two iron bulkheads about 16 ft. from stem and stern respectively, these also being built of flanged plates. The shell has been double riveted throughout. The hatch is 41 ft. long and 17 ft. wide. She is the first craft ever built throughout by hydraulic machinery. The Alpha is to be towed to London after completing her outfit, and is then to be sold for work on the river Thames.

Among those present at the launch were Sir Edward Reed, M.P.; Messrs. J. Nixon, Blyth Shipbuilding Co.; H. H. Wake, River Wear Commissioners; J. T. Eltringham; John Ward, Denny & Co.; C. Doxford, John Campbell; Dr. Stephens; A. C. Holzapfel; M. Holzapfel; J. G. Mellwenna; T. C. Goodall; George Renwick; and others interested in shipbuilding and shipping. In the evening many of these gentlemen were entertained at dinner at the Grand Hotel, Newcastle, by the directors of the company. Mr. Daniel Stephens presided. In proposing the toast of the naval architects, Mr. A. C. Holzapfel pointed out that the little craft had been built with machine labor instead of manual, and that the men in a shipyard had no harder or more exhausting work than that of riveting by hand, which was the work dispensed with.

Sir Edward Reed responded, and in the course of his remarks he said that Mr. Holzapfel's invention presented to the eye of the conservative naval architect many points of radical change. But he must say that

BRIDGE OVER THE SNAKE RIVER AT RIPARIA, WASHINGTON.



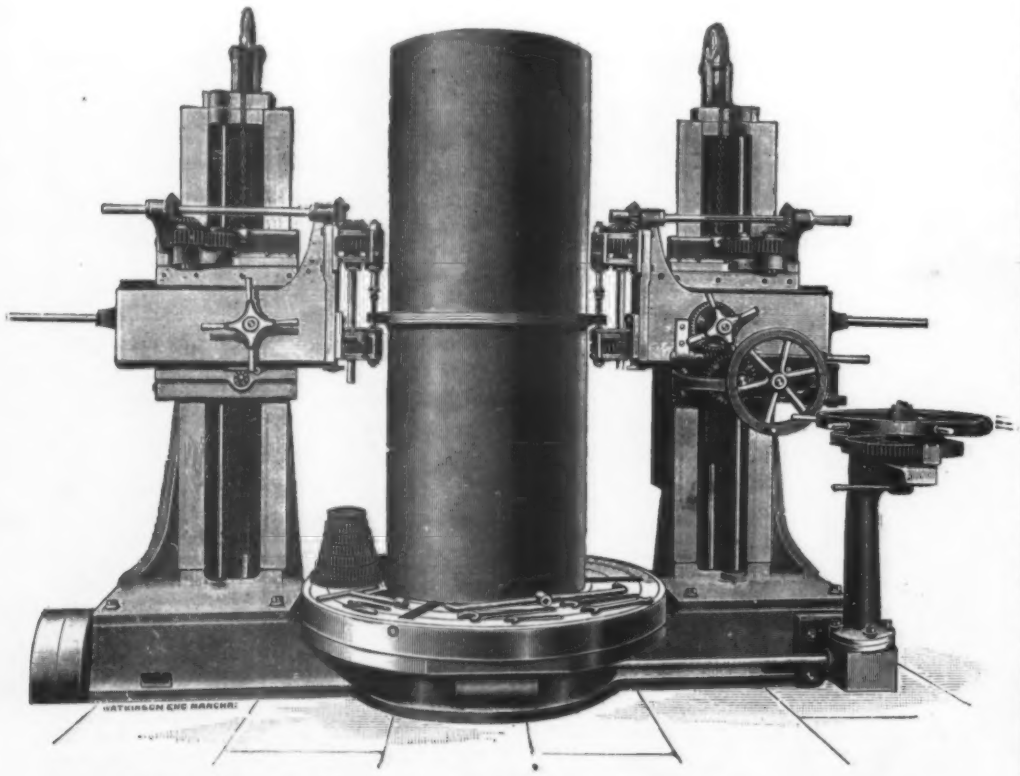


he thought the invention presented points of great importance, and it directed his mind upon certain lines of thought which were not wholly unfamiliar to himself. What many of them were familiar with was the idea that the iron ship as she is at present built is the development of a crude idea. The ultimate idea of a ship ought to be that of a structure which derived its main strength from its main features, viz., its shell, or skin plating, its decks, and its bulkheads. Mr. Holzapfel's craft, whatever further consideration and modification it might require, had the merit of presenting a very stiff structure. He recommended Mr. Holzapfel, in thinking his plan over, to think less of the minuter features of the case, and think more of how he could work his system of hydraulic riveting in with the general system of strength derived chiefly from the skin plating, the decks and the bulkheads. Another line of thought which his plan suggested was that of considering how far a cheapened form of vessel for cargo-carrying purposes would compensate for some other slight attendant disadvantages. He believed a type of ship would be designed some day which would be capable of a very large multiplication at a cheap rate. He thought the simplification of riveting was likely to contribute to that result, and he recommended Mr. Holzapfel to turn his great inventive talent in that direction. He was quite satisfied that the worst features of the boat they saw that day were perfectly capable of being remedied when the invention was more developed. In conclusion, he thanked Mr. Geo. Renwick for showing him the new dock, which he said combined common sense with sound science.

In the course of other remarks, many shipbuilders and shipowners admitted the skillful manner in which the ideas were carried out; and Mr. George Renwick said that for such craft the opening of the Manchester Ship Canal would furnish much employment. The general idea was that modifications would be needed before the plan could be adopted for ocean-going steamers.

BOILER FLUE FLANGE DRILLING MACHINE.

WE illustrate herewith a machine constructed by Messrs. Geo. Booth & Co., Central Iron Works, Halifax, for drilling the rivet holes in position in boiler flue flanges. As shown in the engraving below from *Industries*, the flues are placed on a rotating table, which is fitted with three universal chuck jaws. The table is rotated by a large worm wheel and hand motion, as seen on the right of the illustration. Suitable change wheels are supplied with the machine, in order that any required number of holes can be drilled in various sizes of flanges by simply one turn of the hand wheel, having a pawl, which drops into a notch on the completion of a revolution, thereby preventing the attendant from turning the wheel too far. The two vertical drill saddles are moved simultaneously by the hand wheel shown on the right hand saddle, both having self-acting feed motions. The left-hand saddle, however, can be disengaged when required for adjusting the length of the drill. The twist drills have each an adjustable countersinker for chamfering the top side of the holes; and two other countersinkers, which are adjustable by the small hand wheel, as seen in the illustration, are supplied for chamfering the under side of the holes. The horizontal shaft on the right hand saddle is arranged to receive a twist drill, and as the horizontal slide is also provided with a self-acting feed motion, this spindle may be used for drilling rivet holes in flues for the cross tubes. Both the saddles are counterbalanced by weights and chains, and steel gearing, etc., provided wherever necessary.



IMPROVED BOILER FLUE FLANGE DRILLING MACHINE.

GAUGES AND LIMITS FOR THE STANDARD M. C. B. AUTOMATIC COUPLER.

SECRETARY CLOUD, of the Master Car Builders' Association, has sent out the following circular and illustrations:

In fulfilling the duties assigned to the executive committee by the association in regard to gauges and limits for the standard contour and measurements of the master car builders' automatic coupler when new, the committee has carefully reconsidered the whole question since the discussion on its report to the convention in June, and in view of this discussion and the action of the convention upon its report the committee believes that it is the desire of the association to have it act under the instructions of the convention of 1890, and to make arrangements whereby all parties interested may be able to procure sets of gauges; so that all sets will be alike, which can be used to determine whether any and all new couplers of this type are near enough the standard contour established by the association to insure proper coupling with one another, in so far as it can be insured by close adherence to the standard contour, and also to establish limits of variation for such of the standard rectilinear measurements of the coupler only as will promote the interchangeability of couplers in place upon cars.

FIG. 1.

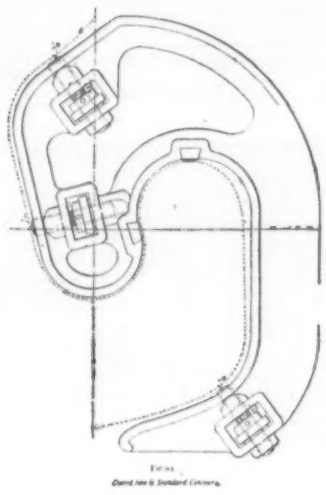
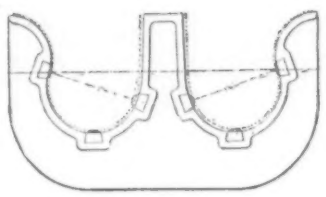


FIG. 2.



The committee therefore announces the gauges shown in Figs. 1 and 2 for the contour line, and thickness of knuckle, respectively, with the limits of variation allowed by these gauges, and it also announces the limits for the standard rectilinear measurements, as given in the table with Fig. 3.

The gauge for new couplers shown in Fig. 1 is announced in lieu of the gauge proposed in the committee's report to the association, because it provides means for gauging the contour lines, excepting the thickness of the knuckle, at points throughout the whole essential extent of the standard form of contour, and it controls the variation in both directions from the standard, whereas the gauge proposed in the report was only a minimum gauge for a portion of the standard contour, and its use would involve numerous measurements at different heights for the maximum variation, instead of gauging for both limits. The figures shown for the variation allowed with this gauge on Fig. 1 are the same as those recommended in the report at the same points, and the additional point on the back of the knuckle is allowed to vary the same as was recommended at the guard arm.

FIG. 3.

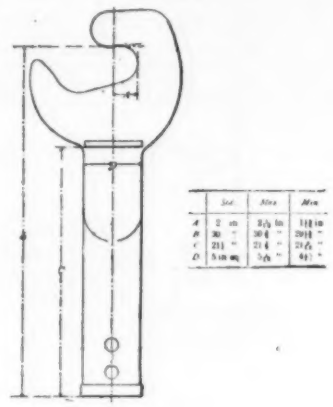
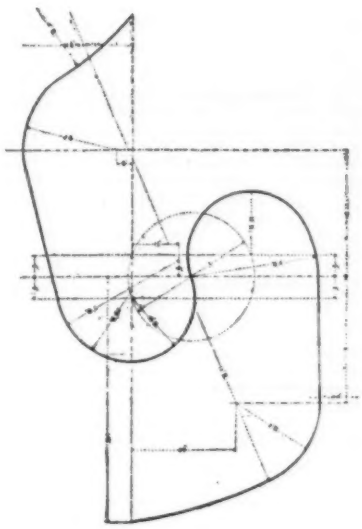


FIG. 4.



The gauge for new knuckles, shown in Fig. 2, is announced as a proper gauge for knuckles, allowing $\frac{1}{16}$ inch variation each way from the standard dimension of 3 inches instead of $\frac{1}{16}$ inch one way only, as recommended in the report, because it is thought desirable to allow more than $\frac{1}{32}$ inch variation, on account partly of the necessary taper in cast knuckles.

The limits shown in table with Fig. 3 are announced as proper limits of variation for the standard rectilinear measurements, which are the same as the limits recommended in the report, except that the standard distance A of 2 inches is included herein with an allowable variation of $\frac{1}{16}$ inch each way, and the cross section D is allowed to vary $\frac{1}{32}$ inch each way from the standard measurement of 5 inches, instead of only $\frac{1}{16}$ inch one way, as recommended in the report. The executive committee considers it inexpedient to announce limits of variation for dimensions which are not standard, but which were mentioned in the report, because the convention ordered that a committee be appointed to report next June on any new standard measurements and limits which may be desirable in connection with the coupler. It is also considered undesirable to provide any specific measuring instrument for the limits of rectilinear measurement given in table with Fig. 3, as these distances may be measured in numerous ways by whatever means are best available.

Fig. 4 shows the standard contour lines, for convenience of reference.

The executive committee has made arrangements with the Pratt & Whitney Company, of Hartford, Conn., to furnish gauges as shown in Figs. 1 and 2, so that every set furnished will be like every other set and proved by master gauges provided and held by the Pratt & Whitney Company for that purpose, a duplicate set of which master gauges will be filed in the office of the secretary of the association for use in setling any questions in dispute about the accuracy of gauges. The frames of the gauges will be made of the best quality of gray iron, with plain lettering, as shown on the cuts, and the gauging points will be of hardened tool steel.—*Nat. Car Builder.*

NEW EDISON PROJECTOR.

THE mirror is 24 inches in diameter. The carbons are set at an angle of 20 degrees from the perpendicular, and are fed by a right and left hand screw. These are actuated by a differentially wound motor, and the whole is controlled by a small relay that starts, stops, or reverses the motors according to the difference of potential in the arc.

A special feature of this lamp is that the mechanism can be detached in a few minutes and entirely removed from the lamp. If anything disables the mechanism it is possible to operate the lamp by hand. It does not matter whether the carbons are actually together or an inch apart, the motor will operate until an arc is established in either case. The movement of the carbons is effected very slowly, so that no flicker or jumping is experienced from sudden movements of the mechanism.

The lamp is designed for a glass mirror on the Mangin principle, which is far more perfect than any system of parabolas. At present a temporary parabolic mirror made of silver plated copper is used, which makes the reflector appear at a disadvantage.

The carbons being set an angle and the negative in advance of the positive, about 80 per cent. of the light is sent in one direction, *i. e.*, to the mirror, and of these a cone of light of 95 degrees is utilized. Standing behind the lamp properly inclined and adjusted, nothing can be seen of the crater in the positive carbon, but a shield is provided to cut off the rays from the incandescent points.

In the projector now used (of which drawings are here shown) the carbons are protected in a cone of refractory material, partly to concentrate the heat and partly to obviate draughts of air. The mirror is also arranged so that it can be adjusted to the carbons, instead of adjusting the carbons to the reflector as by older methods.

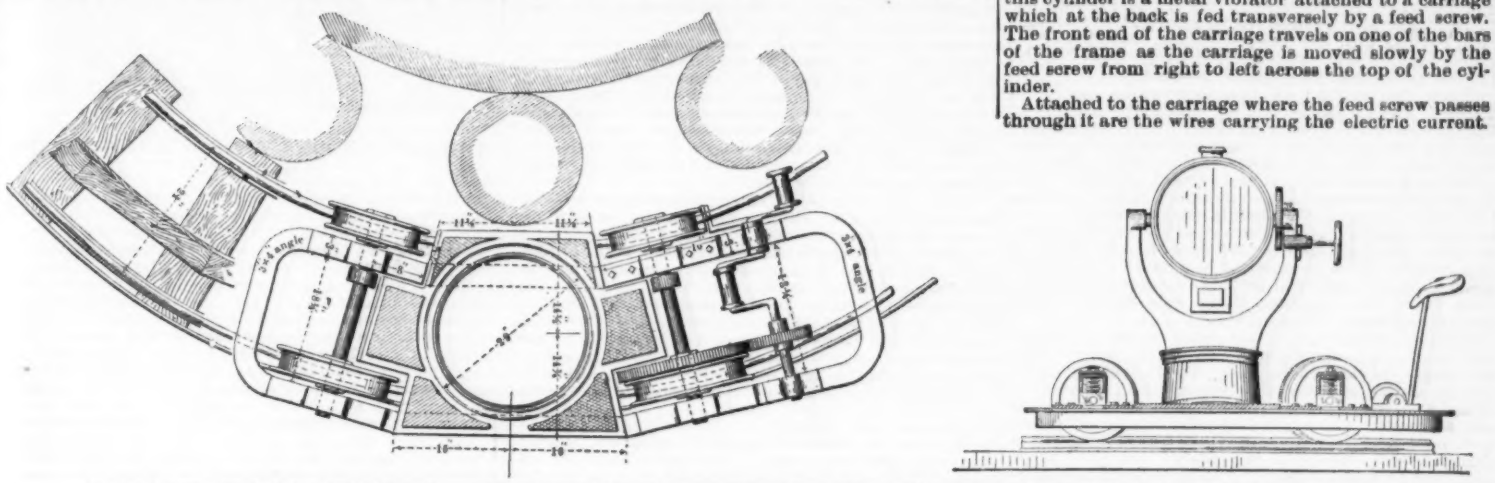
No trouble has been experienced when running currents of 100 amperes or more, which is usually the case without this device, while in addition, it considerably intensifies the light.

Arrangements for observing the arc are very complete. Instead of a blue-tinted glass, through which the operator can see the arc more or less imperfectly and with imminent risk to his eyes, an optical arrangement is used in which a lens throws a distinct image of the arc on a ground glass. This ground glass is furnished with cross lines, so that the centering of the arc can always be effected by the hand wheel shown on top.

The performance of the lamp, under good conditions, on a dark night, free from mist and haze, is to distinctly show up a light colored object at 5,000 yards. For an automatic device, it is the largest permanent reflector ever installed in this country. Up to the present, owing to the optician's delay in sending the mirror, and the failure to obtain the proper carbons, the lamp has been working to a disadvantage; but has, notwithstanding this, created no little comment in the New York papers, and serves the purpose for which it was designed.

This reflector operates on a circular track around the tower of the Madison Square Garden building, New York, at a height of 340 feet. It is mounted on a carriage designed by Mr. J. Van Vleck, of the New York Edison Company, a good idea of which can be obtained from the artist's sketch. The operator works a bicycle arrangement that propels the car. Current for the lamp is taken from a center rail by a brush bearing against it, returning thrugh the wheels to the track. The current to operate the motor is taken from a side trolley, the rails being again used as a common return. A special feeder to supply the necessary current is run from the switchboard in the basement of the amphitheater.

The projector itself was designed by Mr. Robert S. Dobbie, of the Edison General Electric Company, who promises surprising results in light effects when the outfit is complete.—*Electrical World.*



PLAN AND FRONT ELEVATION OF THE EDISON PROJECTOR AND CAR ON THE MADISON SQUARE GARDEN TOWER.

SENDING PHOTOGRAPHS BY WIRE.

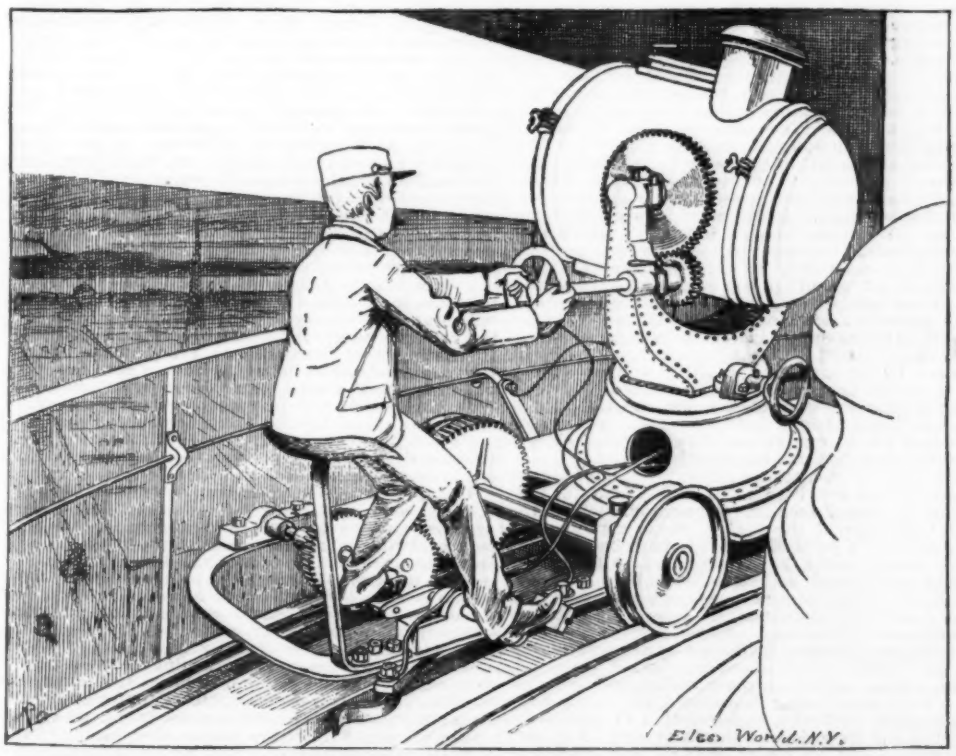
NOAH S. AMSTUTZ, of Cleveland, the inventor of the process for transmitting pictures by electricity, is a little man, with a full beard and wearing iron-bound spectacles. For six long years he has followed his pet idea, until now he feels certain that complete success is about to crown his efforts. Mr. Amstutz's invention is not the first in this line, but none of the others have gone much beyond the visionary stage. He seems to have learned the secret which other inventors sought for in vain. Change is the one word which explains it all. Variation is the better word, for he has learned how to reproduce a variable surface by means of a variable current of electricity, and that discovery is the key to his success. All electricians, and everybody else, for that matter, know that the pressing down of a telegraph key in Cleveland will cause the armature of a sounder in New York or Chicago to click, but it requires only a steady current of electricity to produce that result, and the armature of the sounder moves with each click the full distance allowed to it within prescribed limits. Put into the wires a variable current which will cause an armature to move much or little, to flutter or dance at the will of the operator, and you have solved the problem on which Mr. Amstutz has worked for years, and which he has mastered at last.

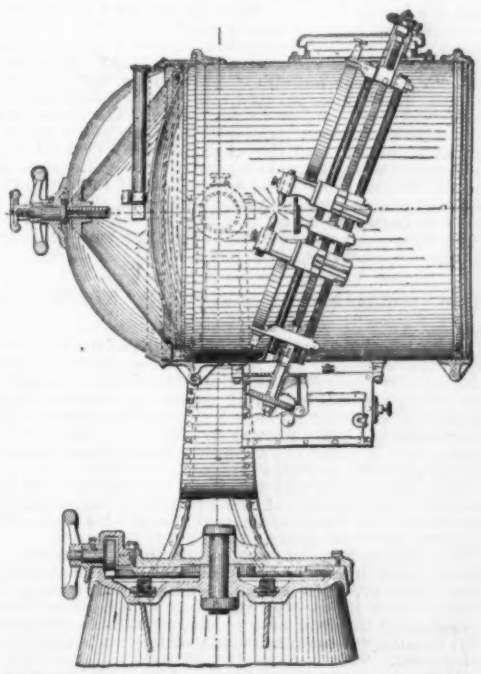
The machine or apparatus on which Mr. Amstutz has labored so long is apparently a very simple mechanical electrical device. It consists of a metal framework about a foot square and much like the frame of a type writer, which supports a brass cylinder three inches in diameter and about eight in length. Above this cylinder is a metal vibrator attached to a carriage which at the back is fed transversely by a feed screw. The front end of the carriage travels on one of the bars of the frame as the carriage is moved slowly by the feed screw from right to left across the top of the cylinder.

Attached to the carriage where the feed screw passes through it are the wires carrying the electric current.

Extending along the lower side of this carriage is the delicate vibrator referred to above, to one end of which, and directly over the brass cylinder, is a tracer that can be raised or lowered by means of a set screw. Suspended over the other end of the vibrator are seven little platinum contacts, against which the vibrator presses as it rises, making connection with the resistances which give variation to the electric current. An electric motor is used to turn the cylinder and feed screw. The process of transmitting the picture seems simpler than the apparatus by which it is done. From a photographic negative a gelatine print is made (being a stripping film). This print has a variable surface, the plane surfaces being white and the various elevations showing greater degrees of shade as they increase in height. The gelatine print, for convenience, is mounted on a strip of celluloid, which is passed around the brass cylinder, and by means of screws is drawn tightly into place, the picture lying around the cylinder with its face up.

When everything is in place, the tracer attached to the vibrator is so adjusted that its point will run smoothly over the lowest surface of the picture, the electric motor is started, and the cylinder begins to move. It revolves about twenty times each minute. The picture passes under the tracer, which, dancing over the variable surface, raises and lowers the vibrator, constantly changing though not entirely breaking the electric current by which the picture is being sent. The feed screw turns very slowly, and the cylinder bearing the picture makes about eighty revolutions while the tracer is moving transversely over an inch of the space along the top of the cylinder. In other words, the tracer passes eighty times from top to bottom of the picture in a space an inch wide, touching every particle of the surface within that space and giving to the electric current all the variations of light and shade as represented by the variable depressions and elevations in the gelatine print. The fineness or coarseness of the work of the apparatus is regulated





GENERAL VIEW AND SECTIONAL ELEVATION OF THE EDISON PROJECTOR.

entirely by different sets of gears. At the other end of the wire is the receiving cylinder, which is exactly synchronized with the one from which the picture is sent, so that the revolutions are the same. On the receiving cylinder, which fits into a metal frame like the other, is a thin sheet of paraffin wax. Adjusted over this sheet of wax is a "V" shaped "graver," or little steel point, attached to a carriage which works on a feed screw similar to that used on the sending cylinder. The current coming over the wires passes through the magnets, having variations corresponding to the variations of light and shade of the photograph being sent. As soon as the electricity is applied at the sending end and the cylinder there is started, the receiving cylinder also begins to move. As the tracer moves up and down over the variable surface of the gelatine film, so the "graver" on the receiving cylinder rises and falls, cutting into the sheet wax, and reproducing in exact detail the variations of light and shade in the picture on the sending cylinder miles away.

When the sending of the photograph has been finished, there is on the wax an exact reproduction of the picture in relief lines, varying in width and depth with the light and shade of the gelatine film from which it was sent. The sheet of wax is then taken from the cylinder, warmed slightly, and pressed out flat. From its surface can be printed proofs of the picture, or from the wax engraving—for that is what it really is—can be made a plaster mould from which a type metal can be cast. The time elapsing between the turning on of the electricity for the sending of a picture two inches square and the casting of the cut ought not to be more than twenty-five minutes. The time will of course vary with the size of the subject.

Sketches, made in half-tone, with variations in light and shade (not simply outline), those approaching the style of an India ink, wood drawing, etc., can be so made under the process that as soon as completed they may be placed upon the cylinder of the transmitter and automatically sent to a distance and there made into an engraving, which is the reproduction itself. This step will enable a special correspondent to send his sketches so as to reach the home office as soon as any press dispatches. It will no doubt be understood that the result arrived at is the correct reproduction of the photograph with all its variations of light and shade, hence its accuracy. The engraving is a special feature of the device. While the result will have an appearance similar to the well-known half-tone engraving, it will not, however, have any of the disadvantages of this process when used for regular newspaper work, namely, the filling up of the lines because of their shallowness, and the ultimate breaking down of them because of their not being strong enough to sustain the heavy pressure of the large cylinder presses. The cutting is done by an electrical-mechanical device, which can be adjusted so as to go a greater or less depth without destroying the fineness of the work. With chemicals this is impossible. A modified receiver is also used, in which the carriage is the same as the one described above, but the support for the material upon which the record is to be made is different, being a reciprocal table having a block of soft metal instead of a cylinder and its sheet of wax. The manner of cutting is different also, because on the metal a rotary cutter is used instead of the "V" shaped graver used on the wax. When the engraving is completed in the metal, proofs can be taken direct, or the block can be stereotyped by the ordinary methods and at once placed in the forms and on the press for printing.

Mr. Amstutz has made several tests of the apparatus, all in private. The first was made on March 27, 1891, and the last on July 17. He chose for his first subject a picture showing a house interior with three persons sitting at a table. There was too much detail in this picture for good work as a first attempt. The next subject was a photograph of Carmencita, the Spanish dancer, whose dress, covered with lace and embroidery, made a difficult surface to work upon. The result was a very good likeness. The last test was upon a photograph of himself, and the result showed decided improvement over the previous tests. The main obstacle now to be overcome is the adjustment of instruments, which must be exact to a hair's breadth. The principle has been conquered, and it is only a question of time when the mechanical difficulties will be overcome.—*Chicago Herald.*

THE MECHANICAL ACTION OF ALTERNATING CURRENTS.

J. Borgman, a member of the Russian Physico-chemical Society, has recently succeeded in reproducing the interesting experiments of Elihu Thomson on the mechanical action of alternating currents in a somewhat different way; and during the course of his researches he has observed several new facts which should not be missed by those who have been attracted to this interesting subject.

The experiments and the facts they elicited are described and discussed at some length in three papers published by Borgman in the *Journal de la Société physico-chimique Russe.*

The alternating current is not indispensable; the same phenomena are produced, but with somewhat less intensity, when a simple intermittent current is employed without ever changing its direction. The presence of the iron core in the coil notably increases the intensity of the phenomena without altering their character.

If upon a vertical coil, traversed by an intermittent or by an alternating current, be placed a cylindrical glass beaker, containing a thin layer of mercury powdered over with lycopodium, then upon the surface of the mercury will be observed two "tourbillons" of opposite character. Any number of these may be produced by interposing under the beaker a disk of copper conveniently cut.

A solution of copper sulphate presents similar phenomena. By means of a telephone it is possible to demonstrate the existence of galvanic currents in movement in the mercury and the existence of points of the same potential.

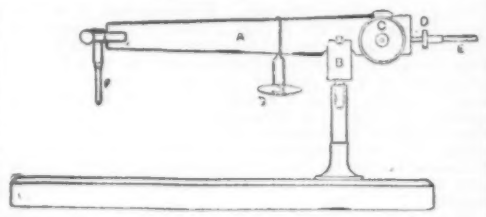
If an alternating current be passed through the mercury in the cylindrical beaker from the center to the circumference, a rotatory movement of the whole mass of the liquid around the center is observed.

The powdering of the surface of the mercury with lycopodium renders the apparatus so sensitive that, if one wishes, the rotation under the influence of the terrestrial magnetic field and of the current furnished by a Grenet element may be observed.

THE RELATIVE HARDNESS OF VARIOUS KINDS OF ELECTRO PLATING.

Few exact experiments have been recorded dealing with the subject to which we now propose to devote a little consideration. The reason for this apparent neglect of what is a matter of some scientific interest, as well as of much technical importance, is to be found in the fact that the accurate determination of the hardness of metals and alloys has only been possible comparatively recently. The fundamental error made by all the earlier devisers of apparatus for the determination of hardness has been that they have overlooked the fact that the hardness of a material is distinct from its tenacity, and methods of measuring the one must be so planned as to be uninfluenced by the other. The failure to appreciate and comply with this condition has been the cause of much wasted ingenuity and effort, as any one will admit who cares to refer to the published accounts of former investigations made in this direction.

A few years ago an instrument was designed by Mr. Thomas Turner, of Birmingham, which avoided the errors of its forerunners almost completely. Like all notable improvements, it owed its excellence to the fact that regard had been paid by its inventor to first principles. Having studied the various methods that had been from time to time proposed, and in some cases actually used in default of anything better, he perceived their inherent defects, and considering precisely what it was that was required to be measured, he abandoned the lines upon which the largest measure of success had been previously attained, and struck out in an entirely new direction. The instrument which was the outcome of his researches is simple enough, as may be seen from the illustration, in which one form of it is shown. A graduated beam, A, with unequal arms like a steelyard, rests upon knife edges which bear upon suitable steel or agate surfaces carried on the head of the pillar, B. A counterweight, C, on



the short side of the beam, serves to balance the longer arm, and final adjustment of equilibrium is made by the nut, D, moving along the screw, E. At the end of the longer arm of the beam is a brass style, F, shod with a small diamond. A scale pan, G, moves along the longer arm of the beam, and by its position and the weights it contains, determines the pressure which the diamond point exercises upon any surface placed beneath it. The pillar, B, has the upper part bearing the beam pivoted vertically to the lower, so that the beam can be swung bodily round in a horizontal plane, thus moving the diamond point in an arc of any desired length. It will be at once apparent that, when a highly polished surface is placed beneath the diamond point and a load of suitable magnitude caused to rest upon it by sliding the scalepan along the beam, on drawing the point across the surface by swinging the beam upon its pivot, an abrasion will be made on the surface if the pressure be sufficient. The load that is necessary to produce this effect is a measure of the hardness of the material under examination. When this instrument is first used by one unfamiliar with the indications it gives, a difficulty is experienced in discriminating between the line made by a pressure on the diamond point, which is insufficient to cut through the surface of the test piece, and a true abrasion. Even with a very light load, particularly in the case of somewhat soft materials, a sort of smoothing of the portion of the surface which has been traversed by the diamond point will be perceptible, and is easily mistaken by the inexpert for a true scratch. When once the use of the sclerometer—for that is the name of the instrument—has been learnt, no difficulty is found in distinguishing between these two appearances, and the determination of the hardness of any given substance becomes comparatively easy.

Considering its merits, it is a little surprising that the sclerometer has not been more widely used for purposes of technical research. A question, such as the relative hardness of the film of metal deposited by electrolysis as a protection to the base body of the article coated, which is acknowledged to be of considerable importance, and has hitherto been subjected to no more fruitful form of investigation than that of vague qualitative discussion, can be settled with ease and accuracy by its means. An instance of what can be done in this direction is afforded by a determination that was recently made of the hardness of three well known plating materials, silver, nickel, and the zinc-silver alloy now being introduced by the London Metallurgical Company, a description of which appeared in *The Electrician* a short time ago. The tests were carried out by Messrs. Stanger and Blount, of Broadway, Westminster, who adopted the use of the sclerometer in their practice as engineering chemists shortly after its appearance.

The results of their investigations are shown in the following table:

Sample.	Condition of Sample.	Hardness.
Nickel anode.....	Rolled.....	11·5
Nickel anode.....	Rolled, annealed....	8·5
Arcas anode.....	Cast.....	5·5
Silver anode.....	Rolled.....	4·5
Silver anode.....	Rolled, annealed....	3·5

The numbers represent the weight in grammes to which the pressure of the diamond point upon the test piece is equivalent. An idea of their value may be obtained from the numbers corresponding to the hardness of a few common metals. Thus, ordinary commercial lead requires a pressure of one gramme on the diamond point to be definitely scratched; tin has a hardness of 2·5; copper about 8; mild steel averages 21; while harder steels and hard cast irons have a hardness of 25·30 or more; and ordinary window glass gives a result of about 60.

The figures are interesting as establishing the claim made for "arcas" that it is appreciably harder than silver. Of course, both are considerably softer than nickel, the hardness of which is well known, and for many purposes objectionable. The value of the testimony in favor of the new method of plating would be much enhanced by the publication of the results of experiments made on the metals in their deposited form, as it is quite conceivable that an alteration in their molecular condition might occur in the act of deposition, and their hardness, among other properties, might suffer change.—*The Electrician.*

PORTLAND CEMENT AND PORTLAND CEMENT CONCRETE.

At a recent meeting of the Institution of Civil Engineers, London, three papers were read relating to Portland cement and Portland cement concrete.

The first was "Portland Cement; its Manufacture, Use, and Testing," by Mr. Henry Kelway Bamber, F.I.C. The author gave analyses of the most suitable clays and chalks to be used in the manufacture of Portland cement, and described the process of preparing and mixing these ingredients, drying the resulting slurry, and of burning. The average composition of good cement was indicated. In these operations care and constant chemical testing were necessary to keep the ingredients in proper proportions, so as to insure the regular percentage of each in the finished cement.

Reference was made to the packing in the kilns of the dried material in lumps of a size not too large to prevent the heat from being evenly distributed through the entire kiln, otherwise there would be a large proportion of insufficiently burned and unclinkered produce. In clinkering, the material entered into semi-fusion only, and it was during this process that the properties of good cement were produced. Given, therefore, proper proportions of ingredients and proper burning, so as to obtain good clinkers, the result would be good cement. This had next to be ground to fine powder and sifted, so that all should pass through a sieve of 2,500 meshes to the square inch, and not less than 90 per cent. should pass through a sieve of 5,625 meshes to the square inch. The finer the cement the more easily was it thoroughly mixed with water, which insured its proper setting without the chance of blowing afterward. About 40 pounds of water should always be used to every cubic foot of cement. This would insure that every portion of cement should have the full quantity it could combine with, and if this were done no blowing could take place, because blowing was caused by portions of cement taking up water and expanding after the body of the cement was set. The author made experiments three years since with concrete composed of four parts of shingle, two of sharp sand, and one of cement. These were mixed in duplicates, the first lot with the full quantity of water it could take, and the others with only half that quantity of water. These blocks after twenty-four hours were put on a wall and were covered twice every day for twelve months by the tide. At the end of the year they were cut through the center. Those made with the full quantity of water were sound, hard, and perfectly dry all through; those made with half the quantity of water were wet through and easily broken to pieces. As there was no difference in any of the ingredients except the quantity of water used, this showed that for concrete to set properly and satisfactorily it must at the outset have the full quantity of water mixed with it. These blocks were made with new cement a day or two old, showing that new cement was perfectly safe if mixed in proper proportions. In the ordinary course cement would be about a month old when used, and there was no advantage in keeping it longer, for it began to lose its power of combining with water, an old cement not being able to absorb and combine with nearly so much water as new cement. In newly ground good cement the particles were in such a molecular state as to quickly combine with water, forming a hard permanent crystallized solid. By age there was evidently a molecular change gradually taking place, for after a time the cement, although not exposed to the air, lost its power of setting when mixed with water, although the percentage composition might be exactly the same.

The author then described the best test to determine if the cement were good: First, there should be a chemical analysis to show the proportions in which each constituent existed, but as this in itself was not sufficient to show if the cement had been fully clinkered, he recommended the taking of the specific gravity, which should not be less than 3·10 to 3·15 at the end of a month. This was much more reliable than the ordinary use of the striked bushel, which varied considerably, according to the way in which the vessel was filled. Then ensued the mixing of the cement with water and the forming into briquettes to test the breaking strain at seven, fourteen, and twenty-eight days; the quantity of water in these briquettes varied with each manipulator, the general desire being to use as little water as possible. The author thought, however, that it should be stipulated how much water should be used in making the briquettes, when the results with the same cement would be more uniform, for a cement that could not take up its full quantity of water, 20 to 25 per cent. of its weight, was not as good as one that could do so, other things being equal. A good cement, not too old, would expand in setting and completely fill the space allotted to it, whereas an old cement would not do so, and consequently spaces would be left for water to enter and weaken it.

The second paper, by Mr. Alfred E. Carey, M. Inst. C.E., was entitled "The Inspection of Portland Cement for Public Works." The author emphasized the following points: Uniformity of the raw materials; completeness of the mechanical blending of the raw materials; the proportion of carbonate of lime; the thoroughness of the wet grinding; the burning; the

flouring; the aeration and packing of the finished cement. Particulars were given of the chemical analysis of various clays in use, and it was pointed out that the hydraulic factors were the silica and alumina, with iron, and that the highest standard of quality was probably attained when the ratio between these

$$\text{was} \frac{\text{Lime}}{\text{Hydraulic factors}} = 2.$$

The various processes for the mechanical blending of the raw materials were described, and special stress was laid upon constant chemical testing at this stage of manufacture, and also on the thoroughness of the blending in a wet state. In regard to burning clinker, the author traced the various chemical changes which took place, and the corresponding temperatures. He considered that a temperature of about 2,900 deg. Fah., maintained for a short time only, was the most favorable for producing high class clinker. He described the more recent systems of burning cement, giving details of the Ransome and Joy processes, and advocated the adoption of the test by specific gravity instead of by weight per bushel. He proposed for this standard, 3·1; a high specific gravity combined with extreme fineness being one of the surest means of proving that a given sample of cement was well burnt. In respect of fine grinding, it was pointed out that it was the flour or impalpable powder which formed the effective part, the nibs or coarse residue forming an adulterant similar to the admixture of a corresponding proportion of sand. This was proved by a series of experiments, in which the residues, on a succession of sieves, were removed from a given sample of cement, when the tensile strains were proportionately increased. The question of the dry grinding and the temperature of cement as it left ordinary millstones and edge runners respectively was then referred to. The author considered aeration essential with cement as now usually manufactured.

With regard to packing, the want of a less expensive and more efficient packing case than the ordinary cement barrel was mentioned. The author suggested that for foreign works clinker might sometimes be conveniently exported and ground on the spot. As to the question of testing, he considered that a test of ½ in. × ½ in. section would in some cases be a useful guide, and described the various apparatus now in use for the purpose of compression testing, calcimeter testing, taking specific gravity, etc. On the question of standard regulations, the rapid progress which had been achieved in quality in England, despite the absence of any government tests, was pointed out, and it was stated that had such a standard been formulated ten years ago, its effect would probably have been retrograde. The practice of foreign governments and societies as to standard tests was detailed, and a specification was given applicable to the present stage of cement production. Minute particulars of the various points to be noted in gauging briquettes were given, and the subject of the adulteration of cement was dealt with. The author did not believe adulteration was much practiced in this country, although the Society of German Cement Manufacturers had thought it desirable to formulate special rules on the subject as regarded Germany.

In dealing with the use of cement, the author stated that, in his opinion, cement and concrete were likely to take the place in many respects of iron and timber as now applied. He also gave particulars of a series of experiments carried out by Mr. Joseph Cash, M. Inst. C.E., at his request, in order to test the power of Portland cement to resist long-continued hydrostatic pressure. These tests were made with concrete of varying proportions, and a further series was carried out to ascertain the extent of the penetration of cement on the surface of a mass of rough concrete. Another series dealt with the question of the extent to which rough concrete would prevent filtration. The efficacy of rendering a surface of concrete in connection with hydrostatic pressure was also discussed. The quantity and purity of water for concrete making was referred to, the author detailing the results of the admixture of fine river mud of varying proportions on the strength of concrete blocks. The effect of the presence of sulphuric acid as observed by experiment was given, as also the effect of frost upon cement briquettes, and *per contra* of high temperatures such as prevail under tropical conditions. The effect of sea water on the maturing of concrete was dealt with, the author considering that when gauged with sea water concrete matured more rapidly and passed more quickly into the crystalline condition than when gauged with fresh water.

The limits within which concrete might be safely applied, more especially on works in the sea, were defined, it being shown that two distinct issues had to be considered in dealing with this much-debated subject: (1) The influence of magnesia as an ingredient in the manufacture of cement. (2) The alleged deterioration of sound cement when subjected to the action of sea water. He showed that caustic lime or caustic magnesia were both harmful in cement, but that a high percentage of magnesia, when clinker was well burnt, was not necessarily deleterious.

In dealing with the second branch of the subject, the author pointed out the desirability of Portland cement not containing more than 4 or 5 per cent. of magnesia, because this material was really an adulterant and not actively useful. He detailed particulars of a series of experiments with cement sand briquettes of varying proportions, which had been exposed to long-continued tidal action without harmful effects, and gave the increase in the weight of these briquettes. He also referred to a series of tests with fresh water, varying proportions of fresh and sea water, and sea water partially evaporated, it being pointed out that in situations in which ammonia might be brought into contact with concrete it was desirable to use fresh water for gauging, and to exclude cement containing any considerable proportion of magnesia. The author's opinion was, however, that no conclusive evidence had been adduced to prove that the precipitates from sea water induced disintegration of even fissured or porous concrete when sound cement was used. In conclusion, the author referred to the chemical and physical transformations of Portland cement in setting, and the general chemical conditions under which Portland cement of the highest class was obtained.

The third paper read was on "The Influence of Sea-water on Portland Cement Mortar and Concrete," by Mr. William Smith, M. Inst. C.E., of Aberdeen. In the course of a discussion which took place at the Institution, in the year 1886, the author, who was the engineer of Aberdeen Harbor, had stated that since the construction of the South Breakwater at that place, in 1872, he had noticed "the disintegration of the surface of the concrete by the chemical action of the salt water on the Portland cement." The softening and swelling of the concrete wing walls at the entrance of the graving dock induced him in the following year to refer the matter to the late Professor Brazier, of Aberdeen University, for a thorough investigation of the chemical reaction causing disintegration. In June, 1887, that gentleman gave his opinion that the analyses of the series of decomposed cements—concrete—showed a remarkable difference in composition from the original cement, inasmuch as in all the samples submitted to him there was found a large quantity of magnesia, and a large proportion of the lime was in the form of carbonate. He believed that this alteration was brought about entirely by the action of sea water upon the cement. There was no other source for either the magnesia or the carbonic acid. The author then reported that "the rapidity of the chemical action was due to the unbalanced pressure of the water on the wing wall" when the dock was emptied. This caused a current of sea water through the porous structure of concrete, which continually washed the decomposed cement into the dock, and brought new particles of concrete into contact with the sea water. In July, 1887, Mr. P. J. Messent, M. Inst. C.E., was requested to make an independent investigation of the cause of the deterioration of the concrete, and in the following November issued a report confirming the results obtained by the author and Professor Brazier, by experiments at the Tyne Piers and by additional analyses of the disintegrated concretes by Mr. Pattinson, the Public Analyst for Newcastle-on-Tyne. From a series of experiments and investigations by Professor Brazier and himself, the author inferred the cause of the expansion of concrete, subject to the influence of sea water, to be the formation of a more bulky compound of magnesia and lime with carbonic acid, and especially with sulphuric acid and water, in place of the lime of the cement. The chemical equations for the action of the lime in the cement upon the magnesian salts in sea water were:

Magnesia chloride + calcic hydrate = magnesic hydrate + calcic chloride.

Magnesic sulphate + calcic hydrate = calcic sulphate + magnesic hydrate, the sulphate of lime crystallizing with water as: Calcic sulphate + water = hydrated calcic sulphate, a substance of great bulk compared with the lime base.

The lime of the cement took up the free carbonic acid from the sea water, forming carbonate of lime; the sulphate, carbonate, and hydrate of magnesia in the sea water were filtered out and united with the lime, forming a deposit of magnesia and hydrated sulphate of lime, which, being bulkier than the lime base, caused swelling and disintegration of the concrete, while the magnesia destroyed the adhesive power of the cement. The author proceeded to point out, from experimental tests carefully made, that the presence of magnesia in the cement, before mixing with water and setting, was not the cause of subsequent disintegration, and that even 5 per cent. of magnesia in the cement only diminished its tensile strength 20 per cent., causing no tendency to disintegration. Sulphate of lime and carbonate of lime might similarly be present in the unslaked cement without much harm or any tendency to subsequent disintegration of the concrete. The disintegration was clearly due alone to the chemical reaction between the sea water and the lime of the cement. All the lime—about 60 per cent.—of the cement was, so far as affected by this action, quite free, there being no stable chemical combination of the lime, alumina, etc., in cement.

A series of experiments on the filtration of sea water from a height of 24 ft. through porous blocks of concrete was given in a tabular form in an appendix, showing, by the gradual decrease of the quantity of water passing through, the closing up of the pores of the concrete by the accumulation of the chemical by-products in the material. The paper concluded with a description of the effect of the compression of air by the inflow of sea water to cavities in the breakwater in bursting out the concrete blocks forming the sides of the cavities. The conclusions arrived at were—that the chief causes of the destruction of concrete sea works were the chemical action of sea water and the sudden compression of air within the structure by waves; and that the same remedy applied to both evils, namely, the formation of an impermeable homogeneous structure.

MANUFACTURE OF STONE WARE.

By EDWARD ORTON, JR.

THE process of manufacturing stone ware is essentially the same in all localities in the United States, the main differences being those which naturally arise from the different scales of working. The successive steps are, 1st, wetting the clay; 2d, grinding; 3d, wedging; 4th, turning; 5th, drying; 6th, slipping; 7th, burning; 8th, sorting the product.

In very many places the clay is put into a bin before using, and allowed to stand over night after drenching with water. This precaution is well taken, particularly in smaller works where horse power only is used in grinding; in the largest steam works it is not necessery. The grinders for stone ware clays are of several kinds; the simplest is a pug mill, which is used in only one place visited. The next machine has no specific name, but is the one in use in all the country potteries. It is a square frame pivoted on an upright beam, which runs through the point of crossing of the diagonals, on the projecting ends of which are fastened cart wheels, which work in a circular trough beneath. The whole frame revolves by the motion of a large cog wheel above, which receives its power from the horse or engine; the motion is slow, but by weighting the corners of the frame, the wheels in their revolutions manage to cut the clay to pieces quite effectually. Such a machine, which can easily be made by any village mechanic, can grind from twelve to fifteen hundred pounds at a charge, and will occupy about two hours in doing it. This quantity of clay will make from 180 to 190 gallons of ware. The clay, after grinding, is balled into large masses and "wet blanketed" to keep until used. The grinding is done in the Akron district in steam mills called tracers, which will be further described under the head of the sewer pipe manufacture. They are very efficient for stone ware clays, grinding about 1,200 pounds to a charge in from 60 to 100 minutes. The clay being ground is put through a process called wedging; it is cut off in balls of 50 pounds or more, and cut by a wire into two parts; these are struck together in such a way that the two pieces unite different faces; the ball is worked up smooth again and is alternately divided and united by cutting halves and striking them together.

This treatment is supposed to eliminate blebs or spaces on the clay and any large pieces of foreign matter. When wedged, it is rolled up into wads or balls which have a definite weight for each kind of ware, and is then ready to be turned. The process of turning is too well known to require description.

The simplest work is making crocks and fruit jars, and after that come jugs, milk pans, churns, etc. The range in size is from one pint to twenty-four gallons, though vessels holding forty gallons have been made in a trial of skill on the part of the turner. As fast as the ware is made it is dried. In the country this is done by setting a flat board covered with ware in the sun. In the smaller potteries of Akron and elsewhere various devices are adopted. The ware is piled into a net work of shelves under which pass steam pipes. The largest and best stone ware works dry it in closets, exactly like those used in the white ware or china works, which will be noticed later.

The dry ware is next "slipped." This technical expression of the potter designates the covering of the ware with any wash or solution. Applied to stone ware, it means the washing of the ware in water containing a very fine-grained clay in suspension, so that its surface is covered by a film of clay. These slip clays on heating vitrify readily to a black glass, and run freely on the ware, making a very handsome glaze. The chemistry of the process is obvious. A partial analysis of the Albany, N. Y., "slip," that is most largely used in the Ohio potteries, shows the following composition:

Sesquioxide of iron ($Fe_2 O_3$)	1·43
Potash ($K_2 O$)	3·17
Soda ($Na_2 O$)	0·74
	5·34

There is present besides a large amount of carbonates of lime and magnesia, which are undetermined. A clay full of the very finest silica and these impurities would, of course, fuse so readily that it would flow like a glaze at the vitrifying heat of stone ware. Slip clay is found in various places in Ohio, but a great deal comes from outside points. Some of these clays fuse to a clear or nearly clear glass, in which case their fusibility is due to alkalies and earths alone. Some slips melt much easier than others, and potters have complained that their slip runs freely only at temperatures so high that the ware begins to lose shape. The judicious use of powdered feldspar, salt water, or carbonate of soda or potash could easily correct this infusibility. The clear slips are best mixed with clays not readily fusible, but which are black in burning. The product has both color and fusibility. The composition of these clays may be farther illustrated by the following analysis of a "kaolite" slip clay, for which thanks are due to Messrs. Whitmore, Robinson & Co., of Akron, Ohio, who use it in their works:

Silica ($Si O_2$)	62·40
Alumina ($Al_2 O_3$)	10·42
Sesquioxide of iron ($Fe_2 O_3$)	5·36
Lime (Ca O)	9·88
Magnesia (Mg O)	4·28
Alkalies (K. Na. Li.)	0·87
Sulphuric acid ($H_2 SO_4$)	0·65
Phosphoric acid (PO_4)	0·19
Carbonic acid and water	8·05
	100·00

Probably not less than 45 per cent. of the silica present is a very fine-grained sand. The main trouble to be overcome in the use of slip glazes is a tendency to blister or "blubber." The cause of this is not definitely known. Every potter has his theory. The opinion of several of the most intelligent has been that undue haste in burning is the most frequent cause, and that the ware should be heated to a low red heat for several hours before any higher heat is attempted, after which rapid raising of the heat can do no harm. This treatment has certainly been successful under the conditions in which it was tested. When the ware is slipped, its capacity is usually stamped on it with a solution of smalt, or cobalt glass, which gives its customary blue color on heating.

The ware is next "set" in the kiln. The kilns in use vary considerably. The country kilns are usually small square structures, bound on the outside by logs of wood; the fire holes are left above the ground and communicate through the various arches of masonry which support the floor of the chamber above; this floor is full of holes, through which the fire freely passes, and the arched roof has usually seven vents of exit for gases. The chamber is oftenest about eight feet in diameter and seven feet high to the arch top. Such a chamber will hold, by nesting the ware, about 1,600 gallons. The firing is commenced at night and is made very light indeed at first; in the morning the ware will be approximately dry, and the heat then continues from twenty to thirty hours, as the conditions vary. Nearly all stone ware is salt-glazed on the outside and slipped inside. The salt is thrown in the fire when the heat is highest, and the vapors are seized by the clay and a soda silicate forms over the surface of the ware. The heat required to do this is the culminating point of the burning. The fuel is nearly always wood; coal is used in some places on a second fire floor lower down than the one used for wood. This method of use will begin to grow as the wood supply fails. The heat applied to stone ware burning is rather severe, as there is always danger of not getting a uniform glaze in different parts of the kiln. The kilns in use at Akron for burning stone ware are examples of the different scales on which the manufacture is conducted.

They all hold 6,000 gallons, and many hold as much as 8,000 and some are even larger. They are oblong kilns about 32 × 16 feet and 12 feet high; the fireplaces are at each end and there are six of them; they are usually down draught, with only one center flue; they are set and drawn from lateral doors, and require seventy hours to burn. A sample is usually withdrawn to test the efficiency of the glazing. Coal is in large use, though wood is still retained in small part. The cooling of a kiln should occupy as much time as the burning. The ware when drawn is piled in storehouses for shipment. 'In viewing a large amount of ware at once, of course the imperfections can be noticed more readily than one piece at a time. One of the most common troubles, besides the "blubbering" of the slip, is the "pitting" of the ware in spots, due to iron. It is a peculiarity of stoneware clay, that while it needs iron to give it color, by far the largest part of the iron present is collected in grains and has no favorable effect on the color. These grains, if the heat is high enough to vitrify them, make pimples on the surface, or break out in rough spots which are black and ill-looking, from the silicate of iron formed. If the heat is too low to vitrify the grain, some change in its volume on heating causes a scale to separate from the surface of the ware, showing in the light-colored cavity the piece of iron oxide at the bottom.

The failure of the ware to glaze is another source of trouble. This may be considered as due to ill-regulated burning usually, but one other cause is thought to act at times. Many clays exhibit collections of fine crystals of a whitish color, which prove on testing to be sulphate of lime or land plaster. This substance when exposed to vapors of salt at high heat suffers a common chemical transformation and changes to chloride of lime, leaving the soda as a sulphate, which it appears either will not displace silica from combination, or will not do it at the temperature the ware will stand without losing shape, and hence there is no glaze. The heat, besides that necessary for the ware, must be high enough to make both slip and salt operate in glazing. The failure of the slip to melt leaves the ware of a rusty red color from the uncombined oxide of iron.

Stone ware should exhibit, on fracture, a blue uniform tint and a semi-vitrified appearance. Different clays act very differently in this respect.

Ware which does not vitrify can be picked out at a glance in most places.—*Brickmaker.*

VANILLA.

PARIS, London, and New York are the markets of the world for vanilla; after these, says Consul Knowles, of Bordeaux, that city occupies an important place. Great quantities of the bean are used in France. It is employed in making confectionery, ices, liqueurs, and cordials, and enters into the manufacture of perfumery. Vanilla is of Mexican origin, though found in almost every tropical country. It is a vigorous rind of the orchid species, about twenty to thirty feet in height, with heavy, oblong, sea green foliage. It thrives in dense forests, where its branches entwine and interlace themselves with neighboring trees. The stem is four-cornered, the flowers large, fragrant, and spikelike. It yields to cultivation. The greater part of the vanilla imported into France comes from her colonies—Guadeloupe, Madagascar (Sainte Marie), Mayotte, the island of Réunion and Tahiti. The production, in 1889, of vanilla in these colonies amounted respectively to 9,000, 19,000, 19,000, 506,000, and 7,000 pounds weight. The vanilla tree begins to yield in its third year, and from thence to its thirtieth. The season commences in the month of April and lasts until the middle of June. The beans are gathered in the green state, before maturity. There are two existing methods of curing vanilla. In the first of these, the beans are laid in quantities on great cloths spread upon the ground, and exposed to the sun for a period of two months, or until they have attained a dark brown color. They are then packed in bundles of fifty, and placed in tin boxes for exportation. The second method consists in attaching, by their inferior extremities, a number of the beans, and immersing the same in a vessel of boiling water, until they become white. They are then exposed for a few hours only to the sun, after which they are covered with a native fixed oil, usually that of the cashew nut. There are four qualities of vanilla, and these are determined by the length and size of the bean, it being found that the flavor and perfume are in direct ratio to their weight and measurement. In appearance the beans are slender and cylindrical, averaging about five or six inches in length and half an inch in thickness. Within is a soft black pulp. The quality of the bean is further enhanced by the exudation of a certain needle-like crystal or efflorescence which covers the surface of the fruit, and is called in French *vanille givree*. The crystals are found to be almost pure benzoic acid. The imports of vanilla into France, which amounted, in weight, to 198,000 pounds in 1880, exceeded 258,000 in 1889.

A NEW GALVANIZING PROCESS.

WE recently inspected a new process for protecting iron and steel, which is being introduced into this country by Messrs. Joseph Westwood & Co., Napier Yard, Millwall, London, E. The metal to be coated is passed through an acid pickling bath, then through an alkaline bath to remove acid, treated with a flux of zinc chloride and stannous chloride, and when thus prepared covered with metallic lead by immersion in a bath of that metal. The point of novelty in the process appears to be the attainment of a close adherence of the lead to the iron by scrupulous attention to cleanliness and the use of specially devised fluxes. The preliminary pickling is accomplished by the passage of a weak current of electricity through the bath in which it is conducted, and on this somewhat slender ground the method has been termed an "electro" lead process. It will be seen that the *modus operandi* and the result attained bear striking resemblance to those of the process of the London Metallurgical Company, which was recently described in these columns. As far as can be judged from the statements that have been made public, the chief difference consists in the use by the latter of a lead alloy and by the former of pure lead. However this may be, the products of the two processes are much alike in appearance, and presumably in properties.

As far as could be seen from tests made of the material at Messrs. Westwood & Co.'s works, the adhesion of the lead coating was remarkably perfect. Sheet iron thus protected was bent double without causing the appearance of any tendency to crack or exfoliate, and specimens of bar were shown which had been twisted spirally upon themselves and were similarly perfect. Sheet metal can also be flanged or corrugated without the coating suffering injury, and the method is also said to be applicable to wire in an equal degree. Wire netting is susceptible of treatment by this method, and the coating it receives is distinctly better in appearance than that produced by galvanizing. The non-tendency of the coating to separate from the metal it is designed to protect is one of the advantages claimed, another of importance being the retention of its tensile strength by the iron or the steel coated, contrasting favorably in this respect with the effect produced by zinc. Indeed, it is claimed that in some cases the strength of the material is enhanced by the coating process, the good effect upon hard steel wire being specially noticeable.

There remains, however, some hope for the galvanizing trade proper. Although lead is much less corrodible than zinc by most of the destructive agents to which coated articles are exposed, yet it by no means follows that the powers of resistance of the articles protected stand in the same order. It is needless to elaborate upon this point; a short consideration of the electro-chemical relations of iron, lead, and zinc will suffice to show wherein the weakness of the new method lies. Zinc is a brittle metal, and one little adapted for a protective coating, but its chemical properties make it the best metallic preservative for iron known, and it will be pushed from its pre-eminence with great difficulty. At the same time there are numerous applications open to lead covering processes such as that we have described.—*Industries.*

BRUNTON'S QUARTERING SHOVEL.

THE author has invented a rapid and accurate sampling tool. The tool consists essentially of a flat bottomed, well balanced steel shovel 10 in. in width, having vertical sides, and two central partitions 2½ in. apart, thus dividing the shovel into three compartments, the center one being closed by a curved back, and having a width one-quarter of the whole.

To use it, the shovel is pushed into a pile of finely crushed ore. As the shovel is raised it is drawn back at the same time with a sharp rotary motion to the right, which throws the ore contained in the outside

SAMPLING SHOVEL.

compartments out from the back end of the shovel into a rejected ore pile. When the necessary throw to accomplish this result has been given, the motion is reversed and the shovel brought rapidly to the left, which action discharges the sample from the central compartment of the shovel upon a pile.

Tests at sampling works and different smelters upon hundreds of lots of ore, many of them in duplicate and triplicate, show that there is no difference between the results obtained by this method and Cornish quartering. Experienced operators attain such great rapidity in this method of quartering that it will doubtless be as applicable to concentrates and all other finely crushed material as to the final reduction of machine samples. In some recent speed tests it was found that a ton of ore could be cut down to a 100 lb. grinder sample by one man in 15 minutes.

PRODUCTION OF OLIVE OIL.

THE olive covers about 70,000 acres in the Department of the Alpes Maritimes, and it yields a revenue of over £400,000. The United States consul at Nice says that there seem to be but two species of olive trees in the south of France, the oleaster (wild olive), having a kind of thorn, very short leaves, and producing only a few small berries, which neither the *dacus* nor the boring caterpillar will attack; the sativa (cultivated olive) is another description, which produces a large fruit. Seedlings of the sativa sometimes deteriorate so as not to be distinguished from the oleaster. There are several varieties of the above, the principal of which are the following: The *blanquetier*, which has a light green foliage, bears a small fruit, and produces oil abundantly. The *blavier* is very hardy; its fruit is oblong, and comparatively large; the pulp is coarse, and the oil deeply colored. The *arabanier*, or *araban*, is a tree hardier than either the *blavier* or the *blanquetier*, and the oil produced from the fruit is of ordinary quality. The *calletier*, which is also known by the characteristic name of *pendoline*, thrives best in dry soil, and, at its best, its fruit sells for a third more than others for mixing with poorer qualities. Other good varieties for oil are the *nirvana*, the *auriola* the *nicoise*, the *blanche*, the *roberon*, the *negrette*, the *caillan*, etc. The olive tree flowers every year; and some growers advocate an attempt to gain a yearly crop, but the majority are content to try to get a good crop every two years. The trees bud in May and flower in June. Olives, to be preserved green, are picked in September; those destined for oil, from November until the following May; but the best results, to crop and tree, seem to follow harvesting near midwinter, when the olive is black, though oil made from olives gathered as late as February and March is preferred for its keeping properties. The farmers in the neighborhood of Nice spread sheets under the trees, and knock the fruit down with poles, injuring both fruit and tree in so doing. The harvest is gathered largely by Italian women, who come into France for that purpose, and they are paid by the quantity gathered, boarding themselves. The olives being gathered, the farmer either takes them to the mill at once or spreads them in the sun or granary to dry out some of the moisture. The mill in use at the present day to crush the olives differs little from those used for centuries. The olives are poured into an enormous stone bowl, from the center of which rises a large wooden shaft crowned with a large wheel; in the rim of the wheel, pointing downward and regularly spaced, are strong oak pegs for cogs. These pegs meet similar pegs or cogs in the power wheel, which is turned slowly by an overshot water wheel. Attached to an upright shaft, forming an acute angle with its lower end, is a large millstone, exactly the shape of a large grindstone, the edge beveled to match the slightly concave bottom of the great bowl, and from the side of the upright shaft opposite to the great millstone, on the end of a short horizontal shaft, is a scraper which fits the inner side of the bowl. When the mill starts, the olives in the bottom of the bowl are crushed by the stone; these, forced up the inner sides of the bowl as the great stone revolves, are scraped off and drop back under the stone until the mass is reduced to an oily paste. This paste is packed into woven grass bags, which are then taken to the presses; these presses are simply rough, heavy frames fitted with large screw presses worked by hand. The bags are then piled up on a wide oaken slab forming the bottom of the frame, the presses are screwed down upon them with a hand-bar, the oil drains into tubs placed to receive it, when boiling water is poured over the bags to help the flow and joins the oil in the tub. The oil rising to the surface of the water is skimmed off with large tin skimmers. A mill has lately been invented, says Consul Bradley, by the director of the agronomic station of Nice, which, as it crushes the pulp, extracts the stone and throws it out, thus allowing the oil of the pulp, the true virgin oil, to be obtained from the press without any admixture of that obtained from the stone or kernel. In preparing virgin oil, which, as it requires so much care and attention to prepare, is difficult to obtain, the following system is adopted. Olives are taken when only three-quarters ripe, and they are selected free from any blemish. They are taken, immediately after they are gathered, to the mill, where they are but slightly crushed, so that the pulp alone comes in contact with the millstone; the seed must not be touched, for though the kernel contains a certain quantity of oil, it is rather acid, and has not so fine a taste as the oil from the pulp. This pulp having been crushed without the addition of water, is gathered in a heap, the center of which is made hollow in the shape of a funnel. The oil flows by itself from the inner sides into the center of a reservoir, from which it is taken with a large ladle. The oil so prepared is greenish in color, its perfume is exquisite, and it can be kept for many years. For oil of the first quality, called "cannon oil," the olives are placed in the mill without addition of water, if the fruit is freshly gathered. The oily paste is placed in bags, made of clean esparto, and put into the press. To obtain second quality oil, and to extract from the pulp all the oil which it contains, the contents of the bags are thrown into a vat full of cold or warm water; the whole is well stirred up, the broken fragments of the seeds fall to the bottom, while the pulp floats, and the latter is gathered and replaced under the press. After all the usual means of extracting the oil from the pulp have been employed, 10 per cent. of oil can still be obtained by using bisulphide of carbon. After the oil is extracted, the skins and refuse are employed in heating boilers; the muddy substance found at the bottom of the most inferior quality of oil is used as manure; and, last of all, the broken stones, or *grignons*, make a very excellent fuel, which has the advantage of not giving off any carbonic acid gas, as charcoal does. Consul Bradley says that fraud is found in the oil mills as in other places. Should the olives be mouldy, which often happens when they have been gathered or kept in bad condition, the bad taste is hidden by adding leaves of wild olive trees to the pulp. Some olive growers even go so far as to throw seed oils over the pulp while it is being ground, so as to get a perfect blending with the new oil. To keep the oil in good condition requires great care and attention. The clear oil is separated from the turbid at once, for the longer the oil remains on the lees the more apt it is to contract a rancid or a bad odor. When the oil has been decanted several times, filtered through dry moss, carded cotton, sand, plaster, charcoal, etc., it is stored in a place which is sheltered both in summer from the heat and in winter from the cold. The oil must be kept in vessels which close tightly, and are made of a substance on which oil has no action. These precautions are indispensable, for the effect of the air on oils is too well known not to be guarded against.

THE HEIGHT OF MOUNT ST. ELIAS.

By MARK B. KERR.

IN a recent dispatch from Washington appears the notice that Professor Israel C. Russell has established his claim that Mt. St. Elias is the highest peak in North America.

As topographer of the expedition of 1890, I feel somewhat responsible for the approximate result obtained by us on this survey, and a few remarks upon the great discrepancy existing in the different estimates, as well as the method used by us in determining its elevation and position, may prove of interest at this time.

Since 1741, when Bering discovered the great peak, there have been seven different estimates made for height and position, and these are tabulated below:

MT. ST. ELIAS.

Date.	Authority.	Height in feet.
1786	La Perouse	12,672
1791	Malaspina	17,851
1847	Tebenkoff	16,938
1872	English Adm. Chart	14,970
1874	U. S. Coast Survey	*19,500
1890	National Geographic Society and U. S. Geol. Survey	15,350
1891	National Geographic Society and U. S. Geol. Survey	18,500

* Within 500 feet.

These different results show a range from 12,672 feet to 20,000 feet.

Up to 1874, the height was obtained by observations from a point at sea, by trigonometric survey, with un-

closed triangles, the sides of the triangles being always long and coefficient of refraction assumed.*

The work of the expedition of 1874 would have forever set at rest all conflict as to height and position, but only one point of the base line was permanently established on land, the other end being far out to sea and not intervisible. The knowledge of this uncertainty led to the expedition of 1890.

In 1890 we measured a small base line, 3,850 feet in length, on permanent ground, deciding it better to have this fixed base and extend our triangulation from it, than to measure a longer base over shifting moraines of dirt and ice.

The accidental loss of the transit made resort to an inferior instrument necessary, and this combined with the region traversed (the line of travel being too direct for a proper development of triangles) would not give the highest value to this determination of position and altitude, but within the approximate trigonometric control the limit of probable error is readily obtained.†

The angles of intersection on Elias are all small. As the ascent of the mountain was not our prime object, we were too late to succeed in reaching its summit, although we missed it by only a few hours.

Combining all angles with the compass angles taken at end of season of 1890 at Port Mulgrave (one end of the assumed base line of expedition of 1874), a distance Mulgrave-Elias is computed 56·16 miles within 5 miles.

This distance was computed in 1874 at 69·10 miles. All this shows that these discrepancies are entirely due to the difference in determination of distance.

It was hoped that the expedition of this season in charge of Mr. I. C. Russell of the Geological Survey would settle the question by succeeding in reaching the summit of the great peak.

But until a set of systematic observations has been approved by the National Geographic Society, the Geological Survey or some well-authorized government or private expedition, doubt will be felt by the scientific world as to its exact height and true position.

So the ambition of mountain-climbers the world over has been provoked by these different failures, and their desire strengthened to take its scalp. But one thing is certain—the American flag should be the first to float over Mt. St. Elias, and may the man who puts it there arrive when the tide of its elevation is on the ebb.—*Mining and Scientific Press.*

[Continued from SUPPLEMENT, No. 831, page 13279.]

THE DECORATIVE TREATMENT OF NATURAL FOLIAGE.‡

By HUGH STANNUS.

Lecture IV.

§ 27.—SHAPES, AND THEIR DECORATION.

THE decoratible SURFACES on which natural foliage is applied are classified, according to their shape, position and function, as:

Panel,
Border,
[Band]
Frieze, and
Indefinite shape and form.

The DECORATION which may be applied to these surfaces is classified, according to its suitability to the above shapes, as:

Panel ornamentation,
Border ornamentation,
Frieze ornamentation,
Diapers,
Body ornamentation, and
Free ornaments.

A PANEL is a portion of surface which is bounded on all sides by Inclosing lines. These lines are or represent the original Constructive Frame; and may be in *color* (as in Surface decoration), or in *relief* (as in Architectural work); and they preclude any extension of the surface.

PANEL ORNAMENTATION may be rich in character. Panels are not a functional part of construction; and they are generally chosen as the place for the most elaborate work and the highest beauty. Their position with regard to the horizon governs the attitude of their ornamentation; when in a *vertical* plane (as on a wall) then it may be in a vertical attitude and axial objects may be introduced; and when *horizontal* (as in a ceiling, table, or floor) then it will be indeterminate in attitude. Panel ornamentation should be complete in each Panel or Set of Panels.

A BORDER is a portion of surface which is divided from, or surrounds the Panel. The surface, from which it is divided, may be horizontal, vertical, or at any angle to the horizon; and the border may be in any attitude on that surface. A border is generally inclosed on its outer and inner edges (*only*), by lines (which are usually *parallel*); and it is capable of extension in a lateral direction.

BORDER ORNAMENTATION should be severe. Borders are generally considered as being analogous to the framing, and thus, being *functional*, should be subordinated to the Panel. Their variability of attitude, further restricts the class of elements, and only permits such as may be reversed in attitude without incongruity. Border ornamentation should be capable of lateral extension, governed by the principle termed Repetition; and of change of direction at the Angles.

[BANDS are mentioned, for the purpose of completing the classification; but they are strictly functional, and will, therefore, not be treated with natural foliage.]

A FRIEZE is a portion of surface which is specially marked off and set apart for enrichment. The surface, from which it is marked off, is always vertical in position; and the frieze is always horizontal in attitude on that surface. A Frieze is always inclosed by lines at its upper and lower edges, which are always parallel; and it is extended laterally.

FRIEZE ORNAMENTATION is generally rich in character. Friezes are not functional, and are often treated with the same elaboration and beauty as Panels. Their invariable verticality of attitude permits a higher class of elements than is appropriate to Borders. Frieze ornamentation should be capable of lateral extension, governed by the principle termed Series, and of change of plane at the Corners.

INDEFINITE SHAPES are those portions of Surface (*i. e.* of *two* dimensions) which are not bounded by Enclosing lines, and which therefore do not belong to any of the classes of Panels, Borders, etc.; *e. g.* a breadth of Wall paper, or of carpet, etc. These are generally treated with either Free ornaments, or Diapers.

INDEFINITE FORMS are those portions of the surface of Objects (*i. e.* of *three* dimensions) which remain when the Friezes, Bands, etc., are divided off. The term is applied more particularly to those Objects which are *not constructed of framing* and which are *circular* or *polygonal in plan; e. g.* the body of a Vase or of an octagonal Pedestal. In one sense they are inclosed by lines, *i. e.* by the Bands round the Body at top and bottom. The surface is however *continuous*, and is, therefore, distinct in character from the Indefinite Shapes, which are primarily flat surface (*only*), and are not required to meet or join in their decoration.

DIAPER ORNAMENTS are those that are capable of indefinite repetition. Each complete unit of the diaper is circumscribed by lines, which form an oblong

FIG. 25.

or a regular geometrical figure. These lines are termed the Repeat lines. It is not necessary that the Pattern figure should show Parallelism to them; more often, *e. g.* in Textile fabrics, the Repeat lines cut through leaf and flower indiscriminately.

BODY ORNAMENTATION may be rich in character on Vases, etc., and reticent on Pedestals, etc. It is not specially functional; and may receive elaborate ornamentation as a high Frieze, or be plain, according to the nature of the Object. Free ornaments are sometimes used on the Bodies of Vases, etc., and occasionally Diapers; but these latter are uninteresting, and often difficult to apply. Decoration which has some *subject* or *meaning* is obviously the best for this.

FREE ORNAMENTS are those whose mass shape is so varied that it cannot be inclosed by simple geometrical lines. They may be *single* (*e. g.* the Tripod support on the Choragic Monument of Lysikrates); or *repeated* (*e. g.* the Wave-scroll above the cornice of the same Monument). They may be seen *against the light* (*e. g.* the above two examples), or *encrusted* over mouldings, etc., or in the centers of panels, examples of both of which are to be seen over the side windows in the ground story of the front of St. Paul's; or otherwise applied in interior decoration. In each of these instances there is no attempt to make the ornament show any parallelism; it is unconfined or *free;* and makes no pretense of filling or occupying the shape of the panel or other space. Any Decorative element which is thus used should be *high* in the scale of elements; and the motive of its application should be *Didactic* rather than merely Æsthetic.

§ 28.—APPLICATION OF DECORATION TO SHAPE.

When natural foliage is applied to an object, for the purpose of Decoration—then it may be so treated as to appear to acknowledge the SHAPE *in* which, and the SURFACE *on* which, it is used.

It may be said to do this in two manners:

(i.) When it is kept within the limits imposed by the Inclosing lines. This is effected by arranging the growth of the branches, leaves, etc., in such a manner that the plant appears to have ceased growing when the extreme leaves approached the inclosing lines; that there is, therefore, neither pushing against and growing over, nor mutilation of the plant, for the purpose of fitting in to the given shape. This quality (and the principle which governs it) is termed PARALLELISM.

(ii.) When it is spread or diffused over the inclosed Surface. This is effected by arranging the direction of the branches with their leaves, etc., in such a manner that no part of the inclosed surface appears bare or unoccupied while another part appears dense and crowded with foliage. This quality (and the principle which governs it) is termed DISTRIBUTION.

§ 29.—PARALLELISM.

PARALLELISM is an important Principle in the application of foliage to inclosed shapes. All decorative art which appears to acknowledge the given shape produces the impression—of having been *made for its place;* and it is thereby more pleasing than if it gave the effect—of being a *portion* that had been cut off from a larger panel.

To attain this: it will be well for the Student *after* the Foliage is drawn in the given Shape, to test the parallelism by drawing lines all round inside the latter and parallel to it at a distance of about a quarter of an inch (this distance depends on Proportion, and it is determined by judgment). He will then be able, should any leaves, etc., overlap this parallel line, to redraw and move them back; and should any not approach it sufficiently near, to bend out the stems. Flowers and leaves may also be turned; and portions may be cut away. These adjustments must be done with judgment, and the natural growth must be always adhered to.

This practice—of bending about or *educating* the branches and stems, so that they may perform their share of the duty in decoration—gives the name to this method of treatment.

The example, Fig. 25, shows a branch of Laurel painted as a study from Nature. It will be seen that it is in Semi-pictorial Realism (as defined in § 11); but it is not yet *decorative* inasmuch as there is no acknowledgment of the framing or limit.

In Fig. 26 the leaves have been "educated," for two reasons: Firstly, to give Clearness (see § 23); and secondly, Parallelism (see § 29). Some of them have been turned, for the purpose of obviating subtile foreshortening (see § 23); and some have been moved, for the purpose of allowing the ground to show through (see § 23). Other leaves have been added at the lower part, for the purpose of occupying the shape (see § 30).

Fig. 27, which is adapted from this, shows further modifications for the purpose of occupying the given

* Coefficient of refraction used in computing heights in this vicinity, determined by Professor George Davidson, U. S. Coast and Geodetic Survey.

† Independent result obtained from Kerr's notes by E. M. Douglas, Topographer U. S. G. S

‡ Lectures before the Society of Arts, London, 1891.

shape, which is a Border. (This is the original of Figs. 7, 8, 9, 18, and 19.)

The Panel, Fig. 4, shows a looseness of effect resulting from the want of more attention to this principle. Some attempt at it will be observed along the top of the panel; but the severe application of the Symmetry makes the looseness of the Parallelism more apparent.

case is evident from two reasons: (i) The space is at axial points; and (ii) the space is left unfilled by the Ribbon (which is only accessory), and not by the Foliage (which is principal).

§ 30.—Distribution.

Distribution is a principle that is more important

Fig. 26.

Fig. 5 shows an improvement; the parallelism is felt along the top and bottom, and at the sides near each angle; the only places without it being in the *center* of each side, *i. e.* at axial positions.

The eye is pleased at these variations, caused by suspending the Principle, provided they arise *from intention and not from inability*. That this has been the

in those applications of foliage which do not require Parallelism, *e. g.* in Diapers. In these: Evenness of diffusion is necessary; and any larger ground spaces that produce the impression of being "holes" are to be avoided. It has also a certain value in inclosed shapes, especially when varied in density (see § 32).

There are degrees in the extent to which these princi-

Fig. 28.

ples may influence the application; and these are characteristic of the three styles shown in Figs. 28 to 30.

In the example of the Indian style, Fig. 28—the Parallelism is completely carried all round inside the Inclosing-lines; and the distribution is so uniform that all the interstitial Ground spaces are of approximately equal area. This is the one extreme—of evenness and density.

In the example of the Japanese style, Fig. 30—neither Parallelism nor Distribution is seen. This is the other extreme—of irregularity and sparsity.

In the example of the English style, Fig. 29 (adapted from the preceding)—the Parallelism is felt at the angles where the branches approach the Inclosing lines. This is sufficient to give an appearance of *steadiness* to the design, and to show that it has been made for that particular shape, without carrying the foliage closely *all round* it; while the Distribution is varied, the Ground-spaces being of differing areas. This design is well *balanced*; and it appears to occupy a middle place in the scale between the uninteresting "texture" of the Indian and the irregular "spotting" of the Japanese.

The characteristics of the three may be stated briefly:—

The Indian *fills* the shape;

The Japanese *ignores* the shape;

The English *occupies* the shape, combining the Obedience of the first with the Freedom of the second.

The Indian is the treatment which might have been

Fig. 27.

expected from the ancient patient rice-eating millions down-trodden for centuries. It is seen in the exquisite work in the India Museum.

The Japanese with its strong individuality and (*apparent*) irregularity is the result of their artistic schools working on traditional lines. The typical method generally ignores the given shape by placing the Plant-form with a studied appearance of irregularity; and often contravenes it by carrying a branch across the Inclosing-lines; and not seldom the work gives the impression of its being *a small piece cut off from a larger*. The resulting Fancifulness is charming—when the irregular feature is the most important in the composition; when it is so beautiful in execution as to repay the attention which is attracted by the irregularity; when there is no repetition, and when it is seen for the first time. It should be borne in mind, in judging of Japanese work, that the greater portion of their hand-made Flower-work is intended primarily as *Symbols* (each plant and each attitude of the foliage having its meaning and association), and only secondarily as *Decoration*. [It will be remembered that—while Æsthetic work is governed by the shape and surface—Didactic work is concerned with the meaning, and ignores the shape.] When, however, the same quaint features and treatment are mechanically repeated—the practice soon degenerates into an irritating affectation.

Fig. 4 shows bad Distribution: there are vacant spaces in the inner part of the panel, which are unpleasing. If the ribbon were reduced, and the foliage increased in quantity to occupy more at the center and at the lower angles, the effect might be more pleasing.

The above remarks have reference to the Distribu-

tion of the *whole* of the features of the plant over the given Shape. There is, however, a higher kind of distribution—the Placing of the Flowers among the Leaves—which is more difficult. But inasmuch as the Flowers, by their scarcity (as compared with the numerousness of the leaves) and by their color (as compared with that of the leaves) are more conspicuous; the arrangement of them in the given Shape and over the Surface is of the highest importance.

There should be a feeling that they are scattered over every part of the given Shape, though not in positions which would be likely to attract attention. In many cases, the Flowers are at the *top* of the Plant; but, if in any Panel the flowers were at the top (*only*), then there would be a want of distribution. Hence the expedients of short stems and bent stems are resorted to. Other expedients that are less justifiable are also used; and in regard to these it should be remembered that it is the clumsiness causing the detection which is here the crime. The position of the Flowers should be considered, however, in selecting the Plant.

FIG. 29.

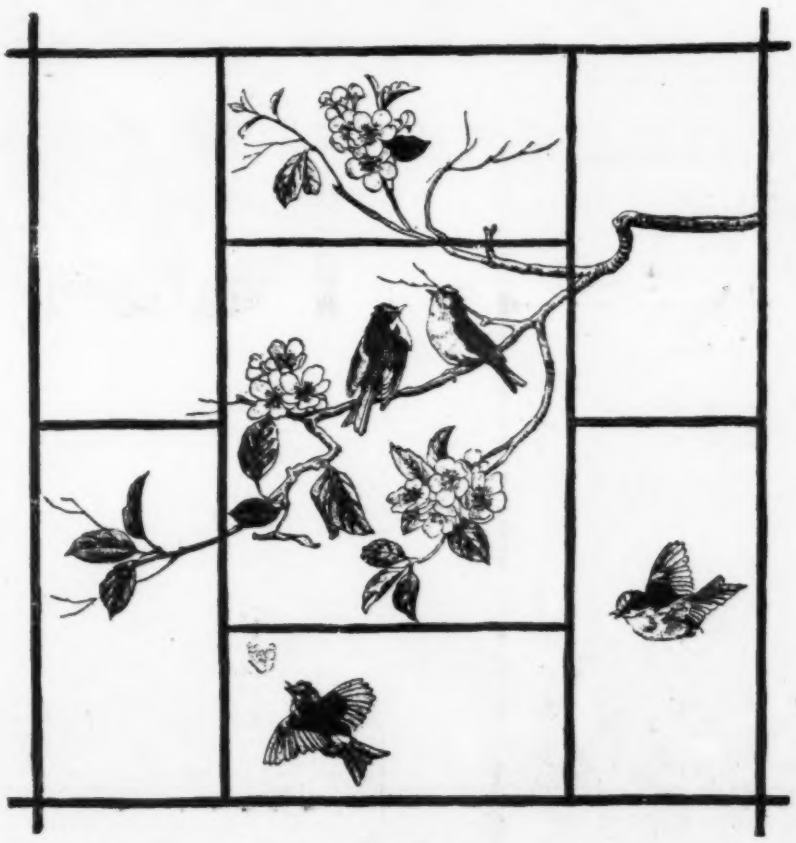


FIG. 30.

§ 31.—PANELS.

PANELS, being never functional, and always important, are not treated so severely as other shapes. A Panel is usually the largest space in an object. It allows of the greatest variety of treatment in Distribution, etc.; and demands the highest beauty of Realism or of some degree of it. It is never *flush* with the Framing or general surface of the object, but usually recessed; and this loss of coincidence-of-surface seems to allow of the still further (apparent) loss or annihilation of surface by the Pictorial treatment. The Inclosing lines seem to necessitate some attention to Parallelism; but it should not be so severely applied in Panels as in the more functional Borders.

In PANEL ORNAMENTATION any appearance of Artificiality in position or attitude is to be carefully avoided.

Repetition is never appropriately applied to the high class features which are used in panels.

The attitudes of leaves near the Enclosing-lines should be varied, to avoid Artificiality, and Monotony.

Flowers afford a good opportunity of obtaining beautiful color in Panels. Artificiality in their Position should be always avoided. In addition to the general rules in § 24, the following treatments should be always avoided:

Flowers on the same vertical line;

Flowers on the same horizontal line;

Flowers in similar positions, *e.g.* in opposite angles.

Parallelism of the Stem to Inclosing lines should always be avoided: it attracts attention; and produces the effect of dividing the panel.

Axiality of the Stem in the center of Panel should be avoided, when possible; and when necessary the stem should be slightly bent, to destroy any possibility of Symmetry.

Proximity of the stems to the inclosing lines should be always avoided. They attract attention; and do not allow space for the foliage to overlap them, as mentioned in § 20.

(*To be continued.*)

SILK DRESSES AND EIGHT HOURS' WORK.

By J. B. MANN.

THE remark occurs in a recent editorial article in a prominent religious newspaper commending the eight hour movement, that if all the women who want silk dresses could have work, all the silk factories in the country could be set in motion and would furnish employment to the many thousands of people then idle; or words of that import. The proposition at first sight seems philosophical, but is it not reasoning in a circle? Having work, people will buy silks. If they buy silks, the factories will run. If the factories run, the people will have work. The old lady said: "This snow will never melt until the weather is warmer, and the weather can never be warmer until the snow has melted." Making the statement does not solve the problem.

When we look at the matter with care we find, sorrowfully, that the women who have no silks are the very ones who do the hardest work; and hence as they are working clear up to the limit of human endurance to get bread, they have no time left over to put into silk dresses. This fact upsets the theory. Horace Greeley had a theory that poverty in cities could be cured by getting the poor to go West and engage in farming; entirely overlooking the fact that the next sixpence the poor man could get, and the next, and so on, must go for bread, thus putting a trip to the West out of the question. But the imagining of philosophers in regard to the remedies is of small account, because want of work is not in this country one of the leading causes of poverty, as every careful observer knows. There are at least a dozen things which are more potent causes of the evil, and too much work, by which constitutions are broken and health ruined, is one of them. Is the remedy, therefore, not to be found in the eight hour movement? I answer, No. The eight hour movement does not approach the root of the evil. It is assumed by the promoters of the movement that society has a given amount of wants which require a given amount of labor to supply, and hence it is inferred that if all the workers cut down their hours from twelve to eight, the men now out of employment will come up and do the work the others have relinquished. In that way it is claimed that there will be work for all. Another theory is that men will accomplish as much in the long run in eight hours as they now do in twelve. It is evident on the face of it that both theories are not true, because if as much should be done by the present workers after the change as before, no more would be left for the others to do than they have now. And in that case the present workers would come much nearer to exhausting their strength and injuring their health for the same money only that they get now. They would be no richer, and would drive their muscles and frames at a wearing pace not consistent with the laws of health.

But neither theory is true. Instead of there being a given amount of wants, as alleged, wants are found to be largely the result of means.

If the community have little, they require little, but as they become wealthy they spread out in proportion. People can't hire labor if they are poor, and hence to make a demand for labor somebody must be rich enough to pay for it. This is perfectly plain. Nobody goes in search of a poor man for employment, only in the last resort. It follows that whatever tends to wealth making tends to want making, and to an increase in the demand for labor and the supply of employment. On the other hand, whatever tends to a diminution of wealth tends necessarily to a diminution of the means to pay for labor, and also to less disposition to hire others to do the work. I think that these positions cannot be successfully combated, and if not, we have a criterion by which to determine in what direction to look for improvement in the condition of the laboring man. Surely we shall never find it in anything that tends to a diminution of resources.

What is stated above in relation to wants being increased in proportion to the increase of wealth does not hold good in some individual cases, but in general it does, and it holds good to that extent that the common people everywhere accept it as a basis of action without stopping to reason about it at all, it is so natural. It is the reason why people leave a country like Ireland and come here. They expect to find dollars so plenty that, according to the old story, they do not deem it worth the while to pick up the quarters they may see lying on the wharf where they land. The same thing takes the smart boys from the poor country districts and small villages to the large towns and cities. They feel that they must get to places where there is an abundance of money. They do not fail to note that a man who has ten thousand dollars will build a three thousand dollar house, while the man with thirty thousand will build a house costing twelve thousand probably, and that calls for four times the labor of the other. They must get where such men abound, and where there are hundred thousand dollar men and millionaires, men who will build palaces, railroads, great warehouses, and ships. Poverty-stricken places are given a wide berth by all sensible folk, and so universal is the practice that we are not left in doubt as to the meaning of it.

Now wealth is principally the product of labor. Some get it by their own labor, and some by the labor of others; but, however got by the individual, it is the result of personal or machine exertion and force. This nessitates the rule, therefore: more labor, more wealth; less labor, less wealth. This rule no one can escape or ignore.

The question now comes up, whether working eight hours a day tends to more riches or more production

than working twelve. That it does not, I have already stated is my belief, and the belief is founded upon a long experience as a mechanic, farm laborer, employer and observer. In twenty years of labor in a shop, I never saw the time when I could do twelve hours' work in eight hours, except possibly for a single day. I never saw the man that could do it, and I never heard of one that could do it.

I never met one that said he thought it could be done for any length of time. It is a well-established fact that most men that pretend to work well have a working ghit of their own, and cannot be hurried beyond that advantageously. If they are, they do poor work or break down. This is so obvious that any pretense that as much will be accomplished in the shorter hours in farming or physical labor of any kind borders on the ridiculous. So obvious is it that the principal advocates of the eight hour movement have ceased to put their case on this ground, and rely upon the other theory, that less work will be done, and consequently more work will be left to be given to the laborers seeking for something to do.

If this latter view be adopted, it follows that the eight hour men are philanthropists, who have sacrificed, or propose to sacrifice, one-third of their possible earnings for the good of their fellow-men who have no work. This is incredible. The laborers themselves do not act from any such principle. They are thinking all the time that, instead of making a sacrifice, they are getting more leisure and making more money. They think that, instead of the work they could do in the four hours they have abandoned being done by the poor fellows who need help, it is not done at all, and, not being done at all, wages have risen, and thus they can get twelve hours' pay for eight hours' work.

In other words, they propose to increase the wealth of the community by lessening the amount produced by the community, thinking that, with a smaller amount to be divided as wages by one third, they can get a bigger share. Not only do they suppose this impossible thing, but they claim it has already been accomplished, and they say the advance in wages during the last thirty years has been caused by the reduction of hours.

Assuming this to be true, it is perfectly legitimate to argue that a further reduction of hours will work in the same way, and they name eight as the next station on the scale, with an intimation that soon six will be the point, and later four. I believe that most concede that it is necessary to have some work done, not perceiving the absurdity into which they fall by the concession. Logically, we say that if one can earn a dollar in one hour, he can earn the same the next hour, and the next, and so on to the limit of his endurance. But, if we begin at the other end of the line of argument, and say that one can do as much and get as much pay in ten hours as in twelve, and then say that he can get as much pay in eight as in twelve, and then again as much in six, there is no logical stop anywhere till the bottom is reached. The stubborn fact of time is kicked out of the back door. It is the same as saying that a man works six hours, earns three dollars, and then works six more at the same work for nothing; while the same persons who say it have to admit that, if the man worked six hours in one day and six hours the next day, he would get as much pay for the second six as for the first six. Time is too tough a customer to be disposed of in that manner, and we must deal with him as a fact that has come to stay.

I think the most stupid are now able to see that one's ability to provide for his wants depends primarily upon his labor, and that time is a principal element in the case. He must have it and he must use it, and his prosperity, other things being equal, will be much or little as time is wisely used or neglected. The law of prosperity has not been repealed by any of the edicts of the leagues and unions. Not a fact or principle has been abolished or suspended. An hour lost is the loss of the product of labor that might have been performed in that hour, and it falls on the man who owned the hour, and not on another man or set of men. He does not escape his loss by the absurd theory that he lost it after four o'clock of Monday, instead of before ten Tuesday morning. It is an absolute loss, whatever the day when it was made. If the man worked for himself, as the saying is, he would see it was a total loss and nothing else; but, working for another, he fancies the other man is the loser, or else, by some hocus-pocus, it is shifted upon society. If men worked by the piece, they would see how it is. Let two men start together in life as shoemakers, with a view to do their best in getting on in the world, as Henry Wilson did sixty years ago. They are equal in skill and endurance, and can work twelve hours at a fair stroke without impairing health. Working by the piece, they find they can earn sixteen and two-thirds cents per hour, or at the rate of two dollars a day. There is no difference between them in purpose, and only the small difference in the method of getting on, that James thinks he will sooner get in comfortable circumstances by working twelve hours a day, and John imagines that nine hours will answer the purpose just as well. At the end of the year of three hundred days they find that James has earned six hundred dollars and John has earned but four hundred and fifty dollars.

They keep on at this rate ten years, and James has laid by two thousand dollars, and John nothing. Now, the two thousand of James earns ten dollars a month for him, and is better than a good apprentice, because he pays the fund no wages and it costs nothing for board. The reason why they are so wide apart is that the extra hours of James have yielded fifteen hundred dollars principal in the ten years, and five hundred dollars in interest. John has nothing, because the expense of living of each and support of the families has amounted to four hundred and fifty dollars a year for each. In ten years more James will have interest money sufficient to meet the family expense of four hundred and fifty dollars, and John will be with his nose still on the grindstone. A company of ten such men would lose in ten years twenty thousand dollars, and society would never make it up to them. Society would not pay for one hundred pairs of shoes when only seventy-five pairs were furnished, and the idea that it would is a delusion. Many workingmen have gained in the last half century, and the general condition has improved a great deal, but no part of the money gain has been due to less hours of labor. The people have grown rich during that time because they have availed themselves of the increased means of production which have been developed, and not because production has been lessened by the laborer refusing to work the former number of hours. Our riches are made up entirely of things produced, and when we say we are richer, we mean that we have more things which are the product of applied force. The increase of wealth, as was stated before, has increased the disposition to build more expensive houses and buy more elaborate furniture, and have an endless variety of things deemed needless a few years ago, causing a demand for labor and an increase of wages that in a measure counterbalances the loss of time. This is what has helped labor, and not the refusal to work more than ten hours. Had the other two hours a day been worked, the laborer would have been still richer by one-sixth of the principal and all the interest on his extra earnings during the whole time that the ten hour rule has prevailed. The workman, then, has simply exchanged the wealth he might have got in the extra two hours for leisure of two hours; a very proper thing to do if he can afford it, but he hasn't had the leisure and the money he might have earned in the lost time also.

The community is also the poorer to the same extent. It misses just the amount of wealth that the laborer has failed to produce in his idle hours. It finds on its hands a large body of men advanced in years who might now be comfortable, but are still struggling to meet the cost of increase in the style of living consequent on the increase of wealth, when they are more than one-sixth short in possible resources.

The trouble with the eight-hour plan, however, is not here so much as in the fact that so many men who cannot get a decent living on eight hours of labor are taught that they can earn as much in that time as in twelve hours, and are made to believe it, or else denounced as scabs and nobodies.

If the laborer attempts to work more hours, he is called an enemy of workingmen, an enemy of progress, and so on, until he is forced to a life of partial idleness, while his children are suffering for comforts which his labor could furnish without injury to himself or to any mortal in the world. There are hosts of men, somewhat deficient in skill, who could partially make up in longer hours their lack of efficiency were they permitted to, but as they are not, they are forced to live on the verge of beggary all their days, and are taught to curse society for not giving them a better chance in the world. How many such there are in this country God only knows, but that they are numerous there can be no doubt. The evil is prodigious, and is not confined to this class entirely. Others are affected in an unfavorable way. The idea is encouraged that labor is an evil to be shunned like vice, and that there is a way to enjoy the fruits of labor without its exercise. The consequence of the prevalence of this idea is that men are led to hope for the impossible, to trust in its coming, and to neglect the golden opportunities for making their way which lie directly before them. The man who thinks he is getting richer by three or four hours of idleness every day is not likely to set much value on time, and when he does that, he tends to unthriftiness, and in time will become a good deal of an idler, if not a downright loafer. When the whole community becomes thus affected, the consequences will be serious. They are serious already.

That this is a remarkable age in which we live is the general belief, but of the things that go to make up this belief nothing is stranger than the fact that when all mankind were devoting their best thoughts to the discovery of ways to increase resources and add to the general and individual wealth of society, when schemes of all sorts were being devised to save time in transportation of goods and mails and persons, in planting corn and making hay, in pumping water and feeding cattle, in tanning leather and making whisky, in mounting flights of stairs and raising broods of chickens—the workingmen as a body should band together and contrive a scheme to compel all hands to throw away absolutely one-fourth of their chances to earn and lay up money, and provide for that period sure to come to all who live out the allotted years of man, when leisure will be not merely a luxury but a necessity; yet this is exactly what they have done. They have in a considerable degree neutralized the gains to themselves to be derived from the use of machinery, and thus have allowed the machines to stand on the pay rolls for the one-quarter of wages they might have earned themselves. It was formerly supposed a wise saying that "The hand of the diligent maketh rich," but the proverb has been strangely modified in these days.

We are now told that the proverb was only three-quarters true, and instead we must say, the man who works all of working time makes his neighbors poor, and will spend his last days in the workhouse of the parish or on the highway as a tramp. Time lost is money lost to the one to whom the time belonged, whether he be rich or poor. The rich can lose some without feeling it, but the poor, alas! have none to spare. When this truth is fully appreciated by the destitute, a long stride will have been made toward the extinction of poverty.—*Popular Science Monthly.*

AN ASTRONOMER'S WORK IN A MODERN OBSERVATORY.*

By Dr. DAVID GILL.

THE work of astronomical observatories has been divided into two classes, viz., astrometry and astrophysics. The first of these relates to astronomy of precision, that is to the determination of the position of celestial objects; the second relates to the study of their physical features and chemical constitution.

Some years ago the aims and objects of these two classes of observatories might have been considered perfectly distinct, and, in fact, were so considered. But I hope to show that in more recent years their objects and their processes have become so interlaced that they cannot with advantage be divided, and a fully equipped modern observatory must be understood to include the work both of *astrometry* and *astrophysics.*

In any such observatory the principal and the fundamental instrument is the transit circle. It is upon the position in the heavens of celestial objects, as determined with this instrument or with kindred instruments, that the whole fair superstructure of exact astronomy rests; that is to say, all that we find of information and prediction in our nautical almanacs, all that we know of the past and can predict of the future motions of the celestial bodies.

Here is a very small and imperfect model, but it will serve to render intelligible the photograph of the actual instrument which will be subsequently projected on the screen. [Here the lecturer described the adjustments and mode of using a transit circle.]

We are now in a position to understand photographs of the instrument itself. But first of all as to the house in which it dwells. Here, now on the screen, is the outside of the main building of the Royal Observatory, Cape of Good Hope. I select it simply because, being the observatory which it is my privilege to direct, it is the one of which I can most easily procure a series of photographs. It was built during the years 1824-28, and like all the observatories built about that time, and like too many built since, it is a fair type of most of the things which an observatory should not be. It is, as you see, an admirably solid and substantial structure, innocent of any architectural charm, and so far as it affords an excellent dwelling place, good library accommodation, and good rooms for computers, no fault can be found with it. But these very qualities render it undesirable as an observatory. An essential matter for a perfect observatory should be the possibility to equalize the internal and the external temperature. The site of an instrument should also be free from the immediate surroundings of chimneys or other origin of ascending currents of heated air. Both these conditions are incompatible with thick walls of masonry and the chimneys of attached dwelling houses, and therefore, as far as possible, I have removed the instruments to small detached houses of their own. But the transit circle still remains in the main building, for, as will be evident to you, it is no easy matter to transport such an instrument.

The first two photographs show the instrument, in one case pointed nearly horizontally to the north, the other pointed nearly vertical. Neither can show all parts of the instrument, but you can see the massive stone piers, weighing many tons each, which, resting on the solid blocks ten feet below, support the pivots. Here are the counterweights which remove a great part of the weight of the instrument from the pivots, leaving only a residual pressure sufficient to enable the pivots to preserve the motion of the instrument in its proper plane. Here are the microscopes by which the circle is read. Here the opening through which the instrument views the meridian sky. The observer's chair is shown in this diagram. His work appears to be very simple, and so it is, but it requires special natural gifts, patience and devotion, and a high sense of the importance of his work to make a first rate meridian observer. Nothing apparently more monotonous can be well imagined if a man is "not to the manner born."

Having directed his instrument by means of the setting circle to the required altitude, he clamps it there and waits for the star which he is about to observe to enter the field. This is what he sees. [Artificial transit of a star by lantern.]

As the star enters the field it passes wire after wire, and as it passes each wire he presses the key of his chronograph and records the instant automatically. As the star passes the middle wire he bisects it with the horizontal web, and again similarly records on his chronograph the transit of the star over the remaining webs. Then he reads off the microscopes by which the circle is read, and also the barometer and thermometer, in order afterward to be able to calculate accurately the effect of atmospheric refraction on the observed altitude of the star; and then his observation is finished. Thus the work of the meridian observer goes on, star after star, hour after hour, and night after night; and, as you see, it differs very widely from the popular notion of an astronomer's occupation. It presents no dreamy contemplation, no watching for new stars, no unexpected or startling phenomena. On the contrary, there is beside him the carefully prepared observing list for the night, the previously calculated circle setting for each star, allowing just sufficient time for the new setting for the real star after the readings of the circle for the previous observation.

After four or five hours of this work, the observers have had enough of it; they have, perhaps, observed fifty or sixty stars, they determine certain instrumental errors, and betake themselves to bed, tired, but (if they are of the right stuff) happy and contented men. At the Cape we employ two observers—one to read the circle and one to record the transit. Four observers are employed, and they are thus on duty each alternate night. Such is the work that an outsider would see were he to enter a working meridian observatory at night, but he would find out, if he came next morning, that the work was by no means over. By far the largest part has yet to follow. An observation that requires only two or three minutes to make at night requires at least half an hour for its reduction by day. Each observation is affected by a number of errors, and these have to be determined and allowed for. Although solidly founded on massive piers resting on the solid rock, the constancy of the instrument's position cannot be relied upon. It goes through small periodic changes in level, in collimation, and in azimuth, which have to be determined by proper means, and the corresponding corrections have to be computed and applied; and, also, there are other corrections for refraction, etc., which involve computation, and have to be applied. But these matters would fall more properly under the head of a special lecture upon the transit instrument. I mention them now merely to explain why so great a part of an astronomer's work comes in the day time, and to dispel the notion that his work belongs only to the night.

One might very well occupy a special lecture in an account of the peculiarities of what is called personal equation—that is to say, the different time which elapses for different observers between the time when the observer believes the star to be upon the wire and the time when the finger responds to the message which the eye has conveyed to the brain. Some observers al-

* Friday evening discourse, delivered at the Royal Institution by Dr. David Gill, F.R.S., Her Majesty's Astronomer at the Cape of Good Hope, on May 29, 1891.—*Nature.*

ways press the key too soon; some always too late. Some years ago I discovered, from observations to which I will subsequently refer, that *all* observers press the chronograph key either too soon for bright stars or too late for faint ones.

Other errors may, and I am sure do, arise both at Greenwich and the Cape, from the impossibility of securing uniformity of outside and inside temperature in a building of strong masonry. The ideal observatory should be as solid as possible as to its foundations, but light as possible as to its roof and walls—say a light framework of iron covered with canvas. But it would be undesirable to cover a valuable and permanent instrument in this way.

But here is a form of observatory which realizes all that is required, and which is eminently suited for permanent use. The walls are of sheet iron, which readily acquire the temperature of the outer air. The iron walls are protected from direct sunshine by wooden louvres, and small doors in the iron walls admit a free circulation of air. The revolving roof is a light framework of iron, covered with well painted *papier mache*.

The photograph now on the screen shows the interior of the observatory, and this brings me to the description of observations of an entirely different class. In this observatory the roof turns round on wheels, so that any part of the sky can be viewed from the telescope. This is so because the instrument in this observatory is intended for purposes which are entirely different from those of a transit circle. The transit circle, as we have seen, is used to determine the *absolute* positions of the heavenly bodies; the heliometer, to determine with greater precision than is possible by the absolute method the *relative* positions of celestial objects.

To explain my meaning as to absolute and relative positions. It would, for example, be a matter of very little importance if the absolute latitude of a point on the Royal Exchange or the Bank of England were one-tenth of a second of arc (or 10 feet) wrong in the maps of the Ordnance Survey of England—that would constitute a small *absolute* error common to all the buildings on the same map of a part of the city, and common to all the adjoining maps also. Such an error, regarded as an *absolute* error, would evidently be of no importance if every point on the map had the same absolute error. There is no one who can say at the present moment whether the absolute latitude of the Royal Exchange—nay, even of the Royal Observatory, Greenwich—is known to 10 feet. But it would be a very serious thing indeed if the relative positions on the same map were 10 feet wrong *here* and *there*. For example, if of two points marking a frontage boundary on Cornhill, one were correct, the other 10 feet in error, what a nice fuss there would be! what food for lawyers! what a bad time for the Ordnance Survey Office! Well, it is just the same in astronomy.

We do not know, we probably never shall know with certainty, the absolute places of even the principal stars to one-tenth of a second of arc. But one-tenth of a second of arc in the measure of some relative positions would be fatal. For example, in the measurement of the sun's parallax an error of one-tenth of a second of arc means an error of 1,000,000 miles, in round numbers, in the sun's distance; and it is only when we can be quite certain of our measures of much smaller quantities than one-tenth of a second of arc that we are in a position to begin seriously the determination of such a problem as that of the distances of the fixed stars. For these problems we must use differential measures—that is, measures of the relative positions of two objects. The most perfect instrument for such purposes is the heliometer.

Lord McLaren has kindly sent from Edinburgh, for the purposes of this lecture, the parts of his heliometer which are necessary to illustrate the principles of the instrument.

This instrument is the same which I used on Lord Crawford's expedition to Mauritius in 1874. It was also kindly lent to me by Lord Crawford for an expedition to the island of Ascension to observe the opposition of Mars in 1877. In 1879, when I went to the Cape, I acquired the instrument from Lord Crawford, and carried out certain researches with it on the distances of the fixed stars.

In 1887, when the Admiralty provided the new heliometer for the Cape Observatory, this instrument again changed hands. It became the property of Lord McLaren. I felt rather disloyal in parting with so old a friend. We had spent so many happy hours together, we had shared a good many anxieties together, *and we knew each other's weaknesses so well.* But my old friend has fallen into good hands, and has found another sphere of work.

The principle of the instrument is as follows. [The instrument was here explained.]

There is now on the screen a picture of the new heliometer of the Cape Observatory, which was mounted in 1887, and has been in constant use ever since. It is an instrument of the most refined modern construction, and is probably the finest apparatus for refined measurement of celestial angles in the world.

[Here were explained the various parts of the instrument in relation to the model, and the actual processes of observation were illustrated by the images of artificial stars projected on a screen.]

Here, again, there is little that conforms to the popular idea of an astronomer's work; there is no searching for objects, no contemplative watching, nothing sensational of any kind. On the contrary, every detail of his work has been previously arranged and calculated beforehand, and the prospect that lies before him in his night's work is simply more or less of a struggle with the difficulties which are created by the agitation of the star images, caused by irregularities in the atmospheric refraction. It is not upon one night in a hundred that the images of stars are perfectly tranquil. You have the same effect in an exaggerated way when looking across a bog on a hot day. Thus, generally, as the images are approached, they appear to cross and recross each other, and the observer must either seize a moment of comparative tranquillity to make his definite bisection, or he may arrive at it by gradual approximations till he finds that the vibrating images of the two stars seem to pass each other as often to one side as to the other. So soon as such a bisection has been made, the time is recorded on the chronograph, then the scales are pointed on and printed off, and so the work goes on, varied only by reversals of the segments and of the position circle. Generally, I now arrange for thirty-two such bisections, and these occupy about an hour and a half. By that time one has had about enough of it, the nerves are somewhat tired, so are the muscles of the back of the neck; and if the observer is wise, and wishes to do his best work, he goes to bed early and gets up again at two or three o'clock in the morning, and goes through a similar piece of work. In fact, this must be his regular routine night after night, whenever the weather is clear, if he is engaged, as I have been, on a large programme of work on the parallaxes of the fixed stars, or on observations to determine the distance of the sun by observations of minor planets.

I will not speak now of these researches, because they are still in process of execution or of reduction. I would rather, in the first place, endeavor to complete the picture of a night's work in a modern observatory.

We pass on to celestial photography, where astrometry and astrophysics join hands. Here on the screen is the interior of one of the new photographic observatories, that at Paris. [Brief description.]

Here is the exterior of our new photographic observatory at the Cape. Here is the interior of it, and the instrument. [Brief description.]

The observer's work during the exposure is simply to direct the telescope to the required part of the sky, and then the clockwork *nearly* does the rest—but not quite so. The observer holds in his hand a little electrical switch with two keys; by pressing one key he can accelerate the velocity of the driving screw by about 1 per cent., and by pressing the other he can retard it 1 per cent. In this way he keeps one of the stars in the field always perfectly bisected by the cross wires of his guiding telescope, and thus corrects the small errors produced partly by changes of refraction, partly by minute unavoidable errors in cutting the teeth of the arc into which the screw of the driving shaft of the clockwork gears.

The work is monotonous rather than fatiguing, and the companionship of a pipe or cigar is very helpful during long exposures. A man can go on for a watch of four or five hours very well, taking plate after plate, exposing each, it may be, forty minutes or an hour. If the night is fine, a second observer follows the first, and so the work goes on the greater part of the night. Next day he develops his plate, and gets something like this. [Star cluster.]

Working just in this way, but with the more humble apparatus which you see imperfectly in the picture now on the screen, we have photographed at the Cape during the past six years the whole of the southern hemisphere from 20° of south declination to the South Pole.

The plates are being measured by Prof. Kapteyn, of Groningen, and I expect that in the course of a year the whole work, containing all the stars to 9½ magnitude (between 200,000 and 300,000 stars) in that region, will be ready for publication. This work is essential as a preliminary step for the execution in the southern hemisphere of the great work inaugurated by the Astrophotographic Congress at Paris in 1887, the last details of which were settled at our meeting at Paris in April last. What we shall do with the new apparatus perhaps I may have the honor to describe to you some years hence, after the work has been done.

We now come to an important class of astronomical work, more purely astrophysical, for the illustration of which I can no longer appeal to the Cape, because I regret to say that we are not yet provided with the means for its prosecution. I refer to the use of the spectroscope in astronomy, and especially to the latest developments of its use for the accurate measurement of the velocity of the motions of stars in the line of sight.*

It is beyond the province of this lecture to enter into history, but it is impossible not to refer to the fact that the chief impulse to astronomical work in this direction was given by Dr. Huggins, our chairman to-night—nay, more, except for the early contributions of Fraunhofer to the subject, Dr. Huggins certainly is the father of sidereal spectroscopy, and that not in one but in every branch of it. He has devised the means, pointed the way, and, while in many branches of the work he still continues to lead the way, he has of necessity left the development of other branches to other hands.

From an astronomer's point of view, the most important advance that has been made in spectroscopy of recent years is the sudden development of precision in the measures of star motion in the line of sight. The method remained for fifteen or sixteen years quite undeveloped from the condition in which it left the hands of Dr. Huggins, and certainly no progress in the accuracy attained by Dr. Huggins was made till the matter was taken up by Dr. Vogel at Potsdam. At a single step Dr. Vogel has raised the precision of the work from that of observations in the days of Ptolemy to that of the days of Bradley—from the days of the old sights and pinnules to the days of telescopes. Therefore I take a Potsdam observation as the best type of a modern spectroscopic observation for description, especially as I have recently visited Dr. Vogel at Potsdam, and he has kindly given me a photograph of his spectroscope, as well as of some of the work done with it.

A photograph of the Potsdam spectroscope attached to the equatorial is now on the screen. [Description.]

The method of observation consists simply in inserting a small photographic plate in the dark slide, directing the telescope to the star, and keeping the image of the star continuously on the slit during an exposure of about an hour; and this is what is obtained on development of the picture.

If the star remained perfectly at rest between the jaws of the slit the spectrum would be represented by a single thread of light, and of course no lines would be visible upon such a thread; but the observer intentionally causes the star image to travel a little along the slit during the time of exposure, and so a spectrum of sensible width is obtained.

You will remark how beautifully sharp are the faint lines in this spectrum. Those who have tried to observe the spectrum of Sirius in the ordinary way know that many of these fine lines cannot be seen or measured with certainty. The reason is that on account of irregularities in atmospheric refraction, the image of a star in the telescope is rarely tranquil; sometimes it shines brightly in the center of the slit, sometimes barely in the slit at all, and the eye becomes puzzled and confused. But the photographic eye is not in the least disturbed; when the star image is in the slit, the plate goes on recording what it sees, and when the star is not in the slit the plate does nothing, and it is of no consequence whatever how rapidly these alternate appearances and disappearances recur. The only difference is that when the air is very steady and the star's image, therefore, always in the slit, the exposure takes less time than when the star is unsteady.

That is one reason why the Potsdam results are so accurate. And there are many other reasons besides, into which I cannot now enter. What, however, it is very important to note is this, that we have here a method which is to a great extent independent of the atmospheric disturbances which in all other departments of astronomical observation have imposed a limit to their precision. Accurate astrospectroscopy, therefore, may be pushed to a degree of perfection which is limited only by the optical aid at our disposal and by the sensibility of our photographic plates.

And now I think we have sufficiently considered the ordinary processes of astronomical observation to illustrate the character of the work of an astronomer at night. The picture should be completed by an account of his work by day; but to go into that matter in detail would certainly not be within the limits of this lecture. It is better that I should in conclusion touch upon some recent remarkable results of these day and night labors. It is these after all that most appeal to you; it is for these that the astronomer labors; it is the prospect of them that lightens the long watches of the night and gives life to the otherwise dead bones of mechanical routine.

Let us take first some spectroscopic results. To explain their meaning let me remind you for a moment of the familiar analogy between light and sound.

The pitch of a musical note depends on the rapidity of the vibrations communicated to the air by the reed or string of the musical instrument that produces the note, a low note being given by slow vibrations and a high one by quick vibrations.

Just in the same way red light depends on relatively slow vibrations of ether, and blue or violet light on relatively quick vibrations. Well, if there is a railway train rapidly approaching one, and the engine sounds its whistle, more waves of sound from that whistle will reach the ear in a second of time than would reach the ear were the train at rest. On the other hand, if the train is traveling at the same rate *away* from the observer, fewer waves of sound will reach his ears in a second of a time. Therefore an observer beside the line should observe a distinct change of pitch in the note of the engine whistle as the train passes him, and as a matter of fact such a change of pitch can be and has been observed.

Just in the same way, if a source of light could be moved rapidly enough toward an observer it would become bluer, or if away from him it would become more red in color. Only it would require a change of velocity in the moving light of some thousands of miles per second in order to render the difference of color sensible to the eye. The experiment is, therefore, not likely to be frequently shown at this lecture table.

But the spectroscope enables such changes of color to be measured with extreme precision. Here on the screen is the most splendid illustration of this that exists at present, viz., copies of three negatives of the spectrum of α Aurigæ, taken at Potsdam in October and December of 1888, and in March, 1889.

The black line (the picture being a negative) represents the bright line $H\gamma$ given by the artificial light of hydrogen, the strong white line in the picture corresponds to the black absorption line which is due to hydrogen in the atmosphere of the star.

Why is it that the artificial hydrogen line does not correspond with the stellar line in all these pictures? The answer is, either the star is moving toward or from the earth in the line of sight, or the earth is moving from or toward the star. But in December the earth in its motion round the sun is moving at right angles to the direction of α Aurigæ; why then does not the stellar hydrogen line agree in position with the terrestrial hydrogen line? The simple explanation is that α Aurigæ is moving with respect to the sun.

In what way is it moving? Well, that also is clear; the stellar line is displaced toward the red end of the spectrum—that is to say, the star light is redder than it should be in consequence of a motion of recession; this proves that the star is moving away from us, and measures of the photograph show the rate of this motion to be 15½ miles per second. We also know that in October the earth, in its motion round the sun, is moving toward α Aurigæ nearly at the same rate as we have just seen that α Aurigæ is running away from the sun. Consequently, at that time, their relative motions are nearly insensible, because both are going at the same rate in the same direction, and we find accordingly in October that the positions of the stellar and artificial hydrogen lines perfectly correspond. Finally, in March, the earth, in its motion round the sun, is moving away from α Aurigæ, and as α Aurigæ is also running away from the sun, the starlight becomes so much redder than normal that the stellar hydrogen line is shifted completely to one side of the hydrogen and artificial line.

The accuracy of these results may be proved as follows:

If we measure all the photographs of α Aurigæ which Dr. Vogel has obtained, we can derive from each a determination of the relative velocity of the motion of the star with respect to our earth.

Of course these velocities are made up of the velocity of motion of α Aurigæ with respect to the sun (which we may reasonably assume to be a uniform velocity) and the velocity of the earth due to its motion round the sun. But the velocity of the earth's motion in its orbit is known with an accuracy of about one five-hundredth part of its amount, and therefore, within that accuracy, we can allow precisely for its effect on

* The older methods enabled us to measure motions at right angles to the line of sight, but till the spectroscope came we could not measure motions in the line of sight.

the relative velocity of the earth and α Aurigæ. When we have done so, we get the following results for the velocity of the motion of α Aurigæ with respect to the sun. You see by the following table how beautifully they agree in the Potsdam results, and how comparatively rough and unreliable are the results obtained by the older method at Greenwich:

α Aurigæ—Potsdam.

Date.	Observed relative motion of earth and star. Miles per sec.	Motion of earth.	Concluded motion. Star relative to the sun.
1888.			
October 22....	+ 2·5	− 13·0	+ 15·5
" 24....	+ 3·1	− 12·4	+ 15·5
" 25....	+ 3·1	− 12·4	+ 15·5
" 28....	+ 2·5	− 11·8	+ 14·3
November 9..	+ 6·8	− 8·7	+ 15·5
December 1..	+ 11·8	− 3·1	+ 14·9
" 13..	+ 14·9	+ 0·6	+ 14·3
1889.			
January 2....	+ 20·5	+ 6·8	+ 13·7
February 5...	+ 32·9	+ 14·3	+ 18·6
March 6.....	+ 34·2	+ 16·8	+ 17·4

α Aurigæ—Greenwich.

Date.	Observed relative motion of earth and star. Miles per sec.	Motion of earth.	Concluded motion. Star relative to the sun.
1887.			
January 26...	+ 16·4	+ 12·6	+ 3·8
February 16..	+ 34·4	+ 15·9	+ 18·5
October 22...	+ 39·8	− 13·5	+ 52·3
" 25...	+ 25·4	− 13·0	+ 38·4
" 29...	+ 40·6	− 12·1	+ 52·7
1888.			
December 7..	+ 29·0	− 1·2	+ 36·2
1889.			
February 15..	+ 23·8	+ 16·0	+ 7·8
March 5	+ 20·3	+ 17·1	+ 3·2
September 17.	+ 18·6	− 13·3	+ 33·3
" 19.	+ 21·8	− 16·7	+ 38·5
" 25.	+ 24·8	− 16·5	+ 41·3
November 25.	+ 24·5	− 4·9	+ 29·4

I believe that in a few years—at least, in a period of time that one may hope to see—we shall not be content merely to correct our results for the motion of the earth in its orbit only, and so test our observations of motion in the line of sight, but that we shall have arrived at a certainty and precision of working which will *permit the process to be reversed*, and that we shall be employing the spectroscope to determine the velocity of the earth's motion in its orbit, or, in other words, to determine the fundamental unit of astronomy, the distance of the sun from the earth.

I will take as another example one recent remarkable spectroscopic discovery.

Miss Maury, in examining a number of photographs of stellar spectra taken at Harvard College, discovered that in the spectrum of β Aurigæ certain lines doubled themselves every two days, becoming single in the intermediate days. Accurate Potsdam observations confirmed the conclusion.

The picture on the screen shows the spectrum of β Aurigæ photographed on November 22 and 25 of last year. In the first the lines are single; in the other every line is doubled. Measures and discussion of a number of these photographs have shown that the doubling of the lines is perfectly accounted for by the supposition of two suns revolving round each other in a period of four days, each moving at a velocity of about 70 miles a second in its orbit.

When one star is approaching us and the other receding, the lines in the spectrum formed by the light of the first star will be moved toward the blue end of the spectrum, those in the spectrum of the second star toward the red end of the spectrum. Then, as two sthrs come into the same line with us, their motions become at right angles to the line of sight, and their two spectra, not being affected by motion, will perfectly coincide; but then, after the stars cross, their spectra again separate in the opposite direction, and so they go on.

Thus by means of their spectra we are in a position to watch and to measure the relative motions of two objects that we can never see apart—nay more, we can determine not only their period of revolution, but also the velocity of their motions in their orbits. Now, if we know the time that a body takes to complete its revolution, and the velocity at which it moves, clearly we know the dimensions of its orbit; and if we know the dimensions of an orbit, we know what attractive force is necessary to compel the body to keep in that orbit, and thus we are able to weigh these bodies. The components of β Aurigæ are two suns, which revolve about each other in four days; they are only between seven and eight millions of miles (or one-twelfth of our distance from the sun) apart, and if they are of equal weight, they each weigh rather over double the weight of our sun.

I have little doubt that these facts do not represent a permanent condition, but simply a stage of evolution in the life history of the system, an earlier stage of which may have been a nebular one.

Other similar double stars have been discovered both at Potsdam and at Cambridge, U. S., stars that we shall never see separately with the eye aided by the most powerful telescope; but time does not permit me to enter into any account of them.

I pass now to another recent result that is of great cosmical interest.

The Cape photographic star charting of the southern hemisphere has been already referred to. In comparing the existing eye-estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Prof. Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky—that is, in the Milky Way—the stars are systematically photographically brighter by comparison with the eye observations than they are in the poorer part of the sky, and that not by any doubtful amount, but by half or three-fourths of a magnitude. One of two things was certain—either that the eye observations were wrong, or that the stars of the Milky Way are bluer or whiter than other stars. But Prof. Pickering, of Cambridge, America, has lately been making a complete photographic review of the heavens, and, by placing a prism in front of the telescope, he has made pictures of the whole sky like this. [Here two examples of the plates of Pickering's spectroscopic *Durchmusterung* were exhibited on the screen.] He has discussed the various types of the spectra of the brighter stars, as thus revealed, according to their distribution in the sky. He finds thus that the stars of the Sirius type occur chiefly in the Milky Way, while stars of other types are fairly divided over the sky.

Now, stars of the Sirius type are very white stars, very rich, relative to other stars, in the rays which act most strongly on a photographic plate. Here, then, is the explanation of the results of our photographic star-charting and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore, we may come to the very remarkable conclusion that the Milky Way is a thing apart, and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch, from the rest of the sidereal universe.

Here is another interesting cosmical revelation which we owe to photography.

You all know the beautiful constellation Orion, and many in this theater have before seen the photograph of the nebula which is now on the screen, taken by Mr. Roberts.

Here is another photograph of the same object, taken with a much longer exposure. You see how over-exposed, in fact burnt out, the brightest part of the picture is, and yet what a wonderful development of faint additional nebulous matter is revealed.

But I do not think that many persons in this room have seen *this* picture, and probably very few have any idea what it represents. It is from the original negative taken by Prof. Pickering, with a small photographic lens of short focus, after six hours' exposure in the clear air of the Andes, 10,000 feet above sea level.

The field embraces the three well-known stars in the belt of Orion, on the one hand, and β Orionis (Rigel) on the other. You can hardly recognize these great white patches as stars; their ill-defined character is simply the result of excessive over-exposure. But mark the wonders which this long exposure with a lens of high intrinsic brilliancy of image has revealed. Here is the great nebula, of course terribly over-exposed; but note its wonderful fainter ramifications. See how the whole area is more or less nebulous, and surrounded as it were with a ring fence of nebulous matter. This nebulosity shows a special concentration about β Orionis.

Well, when Prof. Pickering got this wonderful picture, knowing that I was occupied with investigations on the distances of the fixed stars, he wrote to ask whether I had made any observations to determine the distance of β Orionis, as it would be of great interest to know, from independent evidence, whether this very bright star was really near to us or not. It so happens that the observations were made, and their definitive reduction has shown that β Orionis is really at the same distance from us as are the faint comparison stars. β Orionis is, therefore, probably part and parcel of an enormous system in an advanced but incomplete state of stellar evolution, and that what we have seen in this wonderful picture is all a part of that system.

I should explain what I mean by an elementary or by an advanced state of stellar evolution. There is but one theory of celestial evolution which has so far survived the test of time and comparison with observed facts, viz., the nebular hypothesis of Laplace. Laplace supposed that the sun was originally a huge gaseous or nebulous mass, of a diameter far greater than the orbit of Neptune. I say *originally*—do not misunderstand me. We have finite minds; we can imagine a condition of things which might be supposed to occur at any particular instant of time, however remote, and at any particular distance of space, however great, and we may frame a theory beginning at another time still more remote, and so on. But we can never imagine a theory beginning at an infinite distance of time or at an infinitely distant point in space. Thus, in any theory which man with his finite mind can devise, when we talk of *originally*, we simply mean at or during the time considered in our theory.

Now, Laplace's theory begins at a time, millions on millions of years ago, when the sun had so far disentangled itself from chaos, and its component gaseous particles had by mutual attraction so far coalesced, as to form an enormous gaseous ball far greater in diameter than the orbit of the remotest planet of our present system. The central part of this ball was certainly much more condensed than the rest, and the whole ball revolved. There is nothing improbable in this hypothesis. If gaseous matter came together from different parts of space, such coalition would unquestionably occur, and as in the meeting of opposite streams of water or of opposite currents of wind, vortices would be created, and revolution about an axis set up, such as we are familiar with in the case of whirlpools or cyclones. The resultant would be rotation of the whole globular gaseous mass about an axis.

Now this gaseous globe begins to cool, and as it cools it necessarily contracts. Then follows a necessary result of contraction, viz., the rotation becomes more rapid. This is a well-known fact in dynamics, about which there is no doubt. Thus, the cooling and the contracting go on, and, simultaneously, the velocity of rotation becomes greater and greater. At last the time arrives when, for the outside particles, the velocity of rotation becomes such that the centrifugal force is greater than the attractive force, and so the outside particles break off and form a ring. Then, as the process of cooling and contraction proceeds still further, another ring is formed, and so on, till we have, finally, a succession of rings and a condensed central ball. If from any cause the cooling of any of these rings does not go on uniformly, or if some of the gaseous matter of the ring is more easily liquefied than others, then probably a single nucleus of liquid matter will be formed in that ring, and this nuclei will finally, by attraction, absorb the whole of the matter of which the ring is composed—at first as a gaseous ball with a condensed nucleus, and this will finally solidify into a planet. Or, meanwhile, this yet unformed planet may repeat the history of its parent sun. By contraction, and consequent acceleration of its rotation, it may throw off one or more rings, which in like manner condense into satellites like our moon, or those of Jupiter, Saturn, Uranus, or Neptune. Such, very briefly outlined, is the celebrated nebular hypothesis of Laplace. No one can positively say that the hypothesis is true, still less can any one say that it is untrue. Time does not permit me to enter into the very strong proofs which Laplace urged in favor of its acceptance.

But I beg you for one moment to cast your imaginations back to a period of time long antecedent to that when our sun had begun to disentangle itself from chaos, and when the fleecy clouds of cosmic stuff had but commenced to rush together. What should we see in such a case, were there a true basis for the theory of Laplace? Certainly, in the first place, we should have a huge whirlpool or cyclone of cosmic gaseous stuff, the formation of rings, and the condensation of these rings into gaseous globes.

Remembering this, look now on this wonderful photograph of the nebula in Andromeda, made by Mr. Roberts. In the largest telescopes this nebula appears simply as an oval patch of nearly uniform light, with a few dark canals through it, but no idea of its true form can be obtained, no trace can be found of the significant story which this photograph tells. It is a picture that no human eye, unaided by photography, has ever seen. It is a true picture drawn without the intervention of the hand of fallible man, and uninfluenced by his bias or imagination. Have we not here, so at least it seems to me, a picture of a very early stage in the evolution of a star cluster or sun system—a phase in the history of another star system similar to that which once occurred in our own millions and millions of years ago, when our earth, nay, even our sun itself, "was without form and void," and "darkness was on the face of the deep"?

During this lecture I have been able to trace but very imperfectly the bare outlines of an astronomer's work in a modern observatory, and to give you a very few of its latest results—results which do not come by chance, but by hard labor, and to men who have patience to face dull daily routine for the love of science—to men who realize the imperfections of their methods and are constantly on the alert to improve them.

The mills of the astronomer grind slowly, and he must be infinitely careful and watchful if he would have them, like the mills of God, to grind exceeding small.

I think he may well take for his motto these beautiful lines—

"Like the star
Which shines afar,
Without haste,
Without rest,
Let each man wheel
With steady sway,
Round the task
Which rules the day,
And do his best."

THE UPPER ATMOSPHERE.

By A. C. RANYARD.

I AM enabled this month, through the kindness of Mr. Shadbolt, to lay before the readers of *Knowledge* some photographs taken from balloons at various altitudes. Mr. Shadbolt is a very experienced amateur aeronaut who has made over sixty ascents, and he was, I understand, the first to take a recognizable photograph from a balloon, in June, 1882. The rays of light which fall on a photographic plate exposed from a balloon have had to pass twice through the densest and most dust-laden strata of the atmosphere. Even in photographing distant landscapes from the surface of the ground very little detail is ordinarily obtained upon distant hills, owing to the absorption of the photographically active rays in passing through a great distance of the lower atmosphere. But the difficulty is greatly increased in attempting to photograph the distant earth from a great altitude in a balloon, for not only is the absorption increased by the long course of the rays through the lower air, but the observer sees the dust motes in the atmosphere from their sun-illuminated side. The vale of haze spread over the earth, therefore, hides the objects behind it more effectually than the transparent veil of haze which we ordinarily see over a distant landscape, giving the soft effects of distance which artists so well know.

An observer with a giant telescope, on Mars or on the Moon, would probably see much less of what is going on on the surface of the earth than we are apt to imagine. The white upper surfaces of the clouds would first attract his attention, with, in between them, a very dim and hazy view of objects on the earth's surface, rendered all the more difficult to observe by the frequent presence of a dazzling bright patch of sunlight reflected from the sea. To a naked eye observer on Mars, the earth would probably appear like a variable star, with a curious and mysteriously irregular period of variation, due to the change in the brightness of the specularly reflected patch of sunlight as terrestrial clouds covered it up, or the rotation of the earth brought continents or seas to the part of the earth's surface from which specular reflection could take place.

The exposure of photographs taken from a balloon must necessarily be short, for the balloon generally drifts along with considerable velocity, and sometimes it revolves. The revolution is, however, never very rapid, and it is generally more noticeable as the balloon descends than as it rises, owing to the greater irregularity of the car and under surface of the balloon as compared with the comparatively spherical surface which it presents to the air on rising. With reference to the velocity with which balloons travel, Mr. Shadbolt is of opinion that they do not, as a general rule, travel so rapidly when at a considerable altitude as they do when nearer to the earth. Since this is contrary to the usually received opinion with regard to the velocity of the wind at various altitudes, and Mr. Shadbolt's judgment is founded on very full notes which he takes, during his balloon voyages, as to the time of passing over various places, I give the following extract from a letter he obligingly wrote me on the subject. Mr. Shadbolt says: "I have invariably found the wind near the earth, say up to 1,500 or 2,000 ft., to be stronger than at higher alti-

tudes. I have frequently mounted up to take refuge from an approaching squall, and it has overtaken the balloon and passed below it, while nearly always when above the clouds they appear to travel along at a more rapid speed, and to pass along beneath the balloon. The same thing is noticeable when there are no clouds. You take your bearings and scarcely seem to be moving until you drop down near to the earth. Some might say that this would naturally be due to the distance you are from the objects below, making a quick movement appear to be slow, but after some practice you learn to recognize the actual rate at which you are passing over the ground below you."

At considerably greater heights in the air than balloons have ever attained to, the wind is known occasionally to blow with a velocity of over 140 miles an hour. Thus the dust from the explosion of Krakatoa, which took place on 27th August, 1882, was carried to the West India Islands (half round the earth) in seven days, and the average velocity of the wind at mountain observatories greatly exceeds the average velocity observed at the sea level. A passenger in a balloon hardly feels the wind from the time he starts till the time he touches earth again, for though a high wind may be blowing he is carried along with it, and only feels a slight wind when the balloon, in rising or falling, passes from one current into another, and then the sensation only lasts until the balloon has taken up the velocity of the current of air into which it has passed. The greatest altitude which has been attained by a balloon seems to be a little over seven miles. Mr. James Glaisher and Mr. Coxwell ascended from Wolverhampton on the 5th September, 1862, and are believed to have reached an altitude of 37,000 ft., at which height the barometer would only stand at seven inches. Mr. Glaisher's last reading in ascending was made at a height of 29,000 ft., after that he became insensible, and Mr. Coxwell, at the greatest height attained, lost the use of his hands, and was obliged to pull the cord (which opened the valve and caused them to descend) by seizing it with his teeth and bowing his head downward.

The altitude attained by Messrs. Glaisher and Coxwell, though higher than the highest mountain, sinks into insignificance compared with the total height of the ocean of atmosphere which surrounds the earth.

Small clouds may occasionally be observed at a height of ten miles * above the sea level; and observations of the duration of twilight show that a glimmer of light may generally be traced till the sun is 18° below the horizon, which indicates that there is matter capable of dispersing the sun's light at an altitude of about 40 miles.

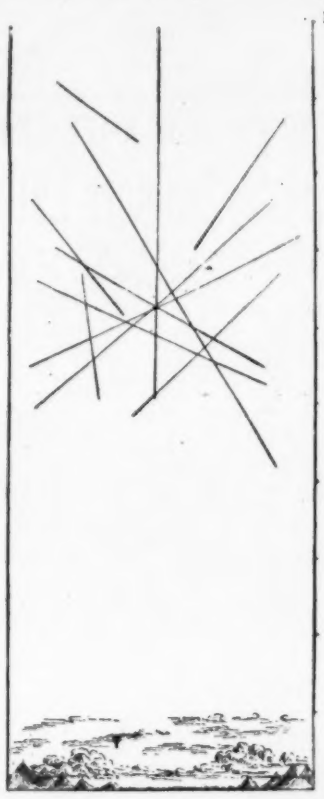
On ordinary occasions the air above that altitude is either too rare, or too pure from foreign particles, to disperse any perceptible amount of twilight. After the explosion of Krakatoa the sunset colors and the longer duration of twilight showed that dust was suspended in the upper atmosphere for many months, at considerably greater altitudes than usual, probably at a height of at least sixty miles. It has been suggested that the volcano threw the dust to an altitude of a few thousand feet, and that it was then carried into the upper air by the rising heated atmosphere of the tropics, which always gives rise to an upcast current carrying the air from near the surface of the ground to a great altitude, and dispersing it and the dust it contains north and south into either hemisphere. But as similar sunset effects were observed in the last century extending over Europe after the great eruption of Skaptar Jokul in Iceland, † it is probable that the heated column of air over the volcano itself would be sufficient to carry dust into the higher regions of the atmosphere.

The smaller shooting stars generally become visible at a height of from 70 to 80 miles above the earth, and they are usually entirely consumed before they penetrate to 50 miles above the sea level, but there is ample evidence that larger meteors have occasionally been observed at a height of over 100 miles above the earth's surface, proving that there is sufficient atmosphere at such great altitudes to resist their motion and convert a part of their energy of translation into heat and light sufficient to render them visible.

The air at these great altitudes must be very rare, much rarer than the vacuum within the bulb of an electric incandescent lamp, for the density of the atmosphere is halved at a height of about three and a half miles, and we know that with an atmosphere composed of gas, which obeys Boyle's law connecting the density with the pressure, the density will continue to be halved with every additional ascent of three and a half miles, consequently at a height of 70 miles the density will have been reduced in the proportion of 1 to 2^{20}, that is, the atmosphere will have less than a millionth of the density of the air at the sea level, which about corresponds to the density of the air within the bulb of an electric lamp (usually spoken of as the vacuum). At a height of 105 miles the density of the air will have been reduced in the proportion of 1 to 2^{30}, or more than a thousand million times.

The reason for this rapid decrease in geometrical progression of the density of the atmosphere as we ascend is easy to see when we consider the pressure on each stratum of three and a half miles in thickness as we rise above the earth's surface. The lowest stratum, which contains a half of the atmosphere, is compressed by the weight of the upper half of the atmosphere, that rests upon it. The next quarter of the atmosphere will occupy a stratum of the same thickness as the lowest half of the atmosphere, because every part of it will be compressed with just half of the weight which compresses a similar stratum of the lowest half, and the atmosphere is composed of gases which very approximately obey Boyle's law, that is, the volume varies inversely as the pressure. In the same manner the third stratum of three and a half miles in thickness will contain an eighth part of the whole atmosphere under the pressure of the weight of the remaining eighth part above it, and the fourth stratum will contain a sixteenth part under the pressure of the weight of the remaining sixteenth part, and so on.

The successive steps at which the density of the earth's atmosphere is halved will in fact become shorter and shorter as the temperature falls in the upper air, for the volume occupied by a mass of gas depends upon its temperature as well as the pressure upon it. While the pressure remains constant the



volume varies as the absolute temperature. Thus, if the height of the lower half of the atmosphere at a temperature of 32° Fahr., or 0° Cent., were exactly three and a half miles, its height at a temperature of T degrees would be $\frac{T}{273} \times 3{\cdot}5$ miles, where T is the absolute temperature measured in degrees Centigrade. We know that the temperature falls very rapidly in the upper air; thus, at the greatest height reached by Mr. Glaisher on the fifth September, 1862, the temperature of the air as registered by a minimum thermometer fell to —11·9° Fahr., at a height where the pressure of the atmosphere was only reduced to about a quarter of the pressure at the sea level. If at a height of 70 miles the temperature falls to —91° Cent., the height of the successive steps of the above series will be reduced to two-thirds of the height of the lowest half of the atmosphere—that is, the density of the atmosphere would be halved at successive steps of a little less than two and a half miles, so that we have considerably under-estimated the actual rate of decrease of the density of the atmosphere on mounting upward.

Let us assume that the density of the air at a height of 100 miles is reduced to a thousand millionth of the density of the atmosphere at the sea level, we are still very far from having got rid of the atmosphere altogether. According to recent estimates, founded on curious corroborative evidence derived from more than one class of physical phenomena, a cubic inch of such tenuous atmosphere would contain about 350 thousand million molecules of gas; a number which is so enormous that its vastness can only be partly realized by means of an illustration. Let us suppose a small bulb of one inch cubic capacity to be filled with air of a thousand millionth, the density of atmospheric air at the sea level. If the bulb were imperfectly sealed so that a thousand molecules could rush into it in every second of time, it would take more than eleven years for the pressure inside the bulb to become doubled—that is, for as many more molecules to rush through the leakage as were originally contained in the bulb.

If the pressure of the air continues to decrease according to the same law, as we proceed upward its density will have been decreased to one million million millionth part of the density at the sea level at a height of 200 miles, and a cubic inch of such air would contain only about 350 molecules; and at a height of less than 222 miles, a cubic inch would only contain one molecule.

At what height should we reach the surface of the atmosphere? Many people, whose opinions are entitled to respect, have thought that the air has not an upper limit. Prof. Young in his "Text-book of General Astronomy," in dealing with the height of the atmosphere (sec. 98), is cautious, and says that it cannot "be asserted positively that the atmosphere has any definite upper limit." But Professor Forster, of Berlin, in a paper which he read before the German Geographical Society * in May last, brings together evidence which he thinks proves the existence of a "Himmelsluft," or thin air prevading in greater or less density the whole of the solar system, and which he assumes is associated with the strata of extremely rarefied gases which follow the earth's movements round the sun. He quotes, as evidence of such a medium, the retardation of Encke's comet when near to perihelion, and the appearance of the Zodiacal light and the "Gegenschein," or "counterglow," which has been so frequently observed, as well as some other evidence which appears to be very doubtful with regard to the height of luminous clouds and the aurora.

The retardation of comets near to perihelion may be satisfactorily accounted for by the great extension of coronal matter about the sun, and the appearance of the zodiacal light and the "Gegenschein" may be due to the light dispersed by flights of meteors which seem to be aggregated near to the plane of the ecliptic and not to be evenly dispersed about the sun, as would be the case if an atmosphere filled inter-planetary space. But the kinetic theory of gases seems to afford evidence that the molecules of the atmosphere do not escape from the region of the earth's attraction. According to the kinetic theory, the molecules of hydrogen, at a temperature of 0° Cent., move with an average velocity of a little more than 6,000 feet per second. Clerk Maxwell, in his "Theory of Heat," 9th edit., p. 314, gives their average velocity as 6,097 feet per second. The atomic weight of nitrogen is 14·01; consequently, assuming Clerk Maxwell's velocity for hydrogen, the molecules of nitrogen at a temperature of 0° Cent. will move with an average velocity of 1,629 feet per second, and the molecules of oxygen, the other chief constituent of the atmosphere, will move a little more slowly. The square of the mean velocity varies as the absolute temperature; consequently as the temperature is lowered the average velocity of the molecules decreases rapidly, and we may feel quite confident, from observations made in balloons and at mountain observatories, that the temperature near to the limits of the atmosphere is far below 0° Cent. We shall therefore be certainly much above the mark in assuming an average velocity of 1,629 feet per second for the molecules of nitrogen in the highest strata of the atmosphere, where their free path becomes very long, and some of them can escape upward without suffering a collision. A projectile thrown upward from such an altitude under the influence of the earth's gravity, with a velocity of 1,629 feet, would be carried to a height of less than nine miles above the point at which it started and would fall again into the atmosphere. It would require a velocity more than twenty-two times as great, or of about seven miles per second, to carry it away from the earth. The velocities above referred to are the mean velocities of molecules at a temperature of 0° Cent. We know that the actual velocities of molecules will be distributed above the mean velocity according to the law of probable error, and it seems very improbable that in the extremely cold upper regions of the atmosphere any molecules will have a sufficiently great upward velocity to be carried outside the region of the earth's attraction.—*Knowledge.*

VEGETARIANISM.

A LADY correspondent of one of our daily papers makes urgent inquiries as to where the vegetarians are, and expresses a desire to form an association and start again this phase of dietetic reform.

To the inquiring mind of the would-be vegetarian we would like to submit the following letter, a portion of which we have reprinted before. The author, Dr. Alanus, gives his reasons for abandoning vegetarianism, and says:

"Having lived for a long time as a vegetarian without feeling any better or worse than formerly with mixed food, I made one day the disagreeable discovery that my arteries began to show sign of atheromatous degeneration. Particularly in the temporal and radial arteries this morbid process was unmistakable. Being still under forty, I could not interpret this symptom as a manifestation of old age, and being, furthermore, not addicted to drink, I was utterly unable to explain the matter. I turned it over and over in my mind without finding a solution of the enigma. I, however, found the explanation quite accidentally in a work of that excellent physician, Dr. E. Monin, of Paris. The following is the verbal translation of the passage in question:

"'In order to continue the criticism of vegetarianism we must not ignore the work of the late lamented Gubler on the influence of a vegetable diet on the chalky degeneration of the arteries. Vegetable food, richer in mineral salts than that of animal origin, introduces more mineral salts into the blood. Raymond has observed numerous cases of atheroma in a monastery of vegetarian friars, among others that of the prior, a man scarcely thirty-two years old, whose arteries were already considerably indurated. The naval surgeon, Treille, has seen numerous cases of atheromatous degeneration in Bombay and Calcutta, where many people live exclusively on rice. A vegetable diet, therefore, ruins the blood vessels and makes prematurely old, if it is true that man is as old as his arteries. It must produce at the same time tartar, the senile arch of the cornea, and phosphaturia.'

"Having unfortunately seen these newest results of medical investigation confirmed by my own case, I have, as a matter of course, returned to a mixed diet. I can no longer consider purely vegetable food as the normal diet of man, but only as a curative method, which is of the greatest service in various morbid states. Some patients may follow this diet for weeks and months, but it is not adapted for everbody's continued use. It is the same as with the starvation cure, which cures some patients, but is not fit to be used continually by the healthy. I have become richer by one experience, which has shown me that a single brutal fact can knock down the most beautiful theoretical structure."

We submit the foregoing statements to the lovers of vegetables, because they show that vegetarianism is not a sure and simple road to health and longevity. We do not, however, find ourselves so completely convinced as does Dr. Alanus, that the premature hardness of his temporal arteries was due to his diet alone. Perhaps he ate too much; for this we are sure is a more certain cause of arterial sclerosis than any special

* Mr. H. P. Curtis, of Boston, tells me that the towering cumulus clouds over thunderstorms on the American prairies may sometimes be seen on the horizon at a distance of 200 miles; proving that they occasionally attain an altitude of over five miles.

† Gilbert White, of Selborne, in one of his letters to the Hon. Daines Barrington, describes "the amazing and portentous phenomena" observed in the summer of 1785. He says the sun "shed a rust colored, ferruginous light on the ground, particularly lurid and blood colored at rising and setting." There are many references to the "red fog" which caused great alarm over Europe during the whole of the summer of 1783. Lalande ascribed it to the effect of a hot sun succeeding a long period of heavy rains, and Cowper refers to it in "The Task," Book II., lines 53-65, which were written in the autumn of 1783; he also refers to the earthquakes and tidal waves of that year. Mrs. Somerville, in her "Physical Geography," traces the origin of these phenomena of 1783 to the great eruption of Skaptar Jokul, which broke out on May 8, and continued till the end of August, sending forth immense quantities of dust as well as lava. Sir John Herschel, in his "Physical Geography," states that Skaptar Jokul ejected 21 cubic miles of lava, a quantity equal to the volume of water poured by the Nile into the sea in a year.

* An abstract of this paper is printed in the "Proceedings of the Royal Geographical Society of London" for July, 1891.

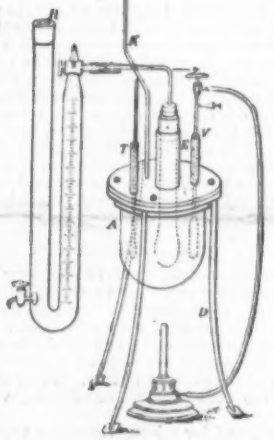
diet. In fact, it is still the custom to put cases of Bright's disease on milk and vegetables, as being more promotive of low arterial tension.

All experience shows that temperance in eating, and a good cook, are better than systems of diet in securing health and longevity.—*Medical Record.*

ANALYSIS OF BEESWAX.

By C. MANGOLD.

AN adulteration of beeswax with less than 6 per cent. of ceresin or paraffin cannot be detected with certainty by any of the ordinary methods, because the relations between the free fatty acid and saponifiable and unsaponifiable matters in genuine beeswax vary within somewhat considerable limits. The detection of smaller quantities of these adulterants can only be made possible by a direct determination of the hydrocarbons present, and the author describes a modification of a method recently brought out by A. and P. Buisine (this journal, 1891, 52 and 729) with that object. The method is based on the observation of Dumas and Stas that if a saponified wax be heated with potash lime, the fatty alcohols are decomposed with formation of fatty acids and evolution of hydrogen, the volume of which becomes a measure of the fatty alcohols present, while the hydrocarbons may be dissolved out of the residue and weighed. The recent improvements of the method consist in the simplification of the apparatus and a more exact knowledge of the conditions to be observed. From 2 to 10 grms. of the wax are saponified by melting with potash lime. The solid dry soap is then powdered and well mixed with three times its weight of potash lime and placed in a strong pear-shaped flask, in which it is heated to 250° for two hours. The apparatus for conducting this operation is shown in the sketch. A is an iron vessel with a lid fastened down by screws, and filled with mercury. The flask or eprouvette, E, is connected, gas tight, with a Hofmann's burette, H, for measuring the hydrogen evolved. T is a thermometer and V a temperature regulator. K is a condensing tube for mercury vapor. The author recommends that the heating at 250° be continued for three hours to secure completion of the reaction, after which the flask is allowed to cool, and is broken up to liberate the residual mass, which is then powdered and extracted with petroleum spirit in a Soxhlet apparatus. The residue left on evaporation of the petroleum spirit is dried at 110° and weighed.



APPARATUS FOR ANALYSIS OF BEESWAX.

Genuine beeswax examined by this method gives always some hydrocarbons, but the experience of the author and other observers is to the effect that the quantity is almost invariably between 12·5 and 14·5 per cent. The mean of these quantities, or 13·5, must therefore be deducted from the total hydrocarbon found, and the difference is the amount of hydrocarbon added by way of adulteration. The results of numerous analyses accompany the article.—*Chem. Zeit., Jour. Soc. Chem. Ind.*

CITRIC ACID IN MILK.

By A. SCHEIBE.

WITH ordinary fodder citric acid occurs in goat's milk as in cow's milk to the extent of 1-1·5 grammes per liter, but the amount is liable to considerable fluctuations, being sometimes double as great as at others (calculating it upon the total solids). It does not appear to be derived from the citric acid present ready formed in the fodder, as it also occurs, through in smaller quantity, in human milk. Moreover, citric acid added to the fodder does not increase the amount found in the milk, and feeding exclusively upon materials free from citric acid has no effect upon the quantity excreted. Milk secreted in a state of hunger contains the normal amount of citric acid.

Citric acid in milk is apparently not derived from the cellulose undergoing digestion in the intestines of herbivora, as excretion goes on when food is withheld or meal substituted for ordinary fodder; this is consistent with the fact that in the human subject (where no digestion of cellulose takes place) 0·54-0·57 gramme of citric acid per liter of milk is formed.—*Landw. Versuchstat, Chem. Zeit.*

CITRIC ACID IN WINE.

By A. KLINGER and A. BUJARD.

IN testing wine in accordance with the Nessler-Barth method, a sample of wine yielded a precipitate of the calcium salt which, on being further tested, was found to be the malate, and not citrate. On investigating the matter, the authors found that malate of calcium is precipitated by boiling from its nearly neutral solution in the same manner as citrate. To distinguish between the two salts, the following method is recommended: At least 250 c. c. of the wine to be tested are evaporated to about one-third of its bulk. Potassium acetate is then added, the solution strongly acidulated with acetic acid, and a double bulk of strong alcohol added. This throws down the tartrate. After twenty-four hours the precipitate is filtered off, washed with a few c. c. dilute alcohol. The filtrate is precipitated with basic lead acetate, the precipitate being also washed with alcohol, it is then decomposed with hydrogen sulphide.

The filtrate from the lead sulphide is concentrated and made alkaline with lime water. The precipitate, which consists of phosphate and a trace of tartrate, filtered off after a few hours. The solution is rendered acid with acetic acid, and evaporated to dryness. The residue is dissolved in a little hot dilute hydrochloric acid, and after addition of some ammonium chloride, is made faintly ammoniacal, and boiled for some length of time, when only citrate will be precipitated, and no malate. (*Zeitsch. f. angew. Chem.* 1891, p. 514). —*The Analyst.*

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